RESEARCHES,

PHYSIOLOGICAL AND ANATOMICAL.

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I.

AN ACCOUNT OF SOME EXPERIMENTS AND OBSERVATIONS ON THE BLOOD.

Many of the following observations and experiments were contained in my inaugural dissertation on the blood, under the title of *Tentamen experimentale quædam de Sanguine conplectens*, written on the occasion of graduating, in 1814.

The views taken of the composition of the blood which I then adopted, and have not yet seen any reason, founded on facts, for relinquishing, were at that time, I believe I may say, commonly taught in the schools, at least in Edinburgh, and received as well-substantiated doctrines, resting chiefly on the researches of the great physiologists of our own country, and especially of Hewson.

In making this remark, of course I allude to the general and broad views of the subject; not to details and special views deducible from particular experiments, to which, considering the nature of the blood, and the accumulated experience of ages relative to the investigation of its properties, there seems to be hardly any limit: at least, we cannot at present discern the limit.

One of the distinguished physiologists of the 17th century, Lower, in the dedication of his very original work, *De corde, item de Motu et Colore Sanguinis*, appears to have thought it necessary to offer some apology to his friend for writing on such a topic :—"Mirabuntur alii, forte et ipse miraris, vir ornatissime, de corde et sanguine post viros celebres, qui materiam hanc non tantum tractasse sed et exhausisse videantur, a me quicquam amplius proferri." How different is the feeling now; instead of exhausted, the inquiry seems, humanly speaking, rather inexhaustible.

1.—*Observations to endeavour to determine whether any heat is evolved by the blood in coagulating.*

Although the remarkable property of coagulation which the blood possesses has been laboriously and carefully investigated, yet, it must be confessed, we know but little respecting it; it is still one of the many mysteries of nature. The little we know on the subject, appears to be chiefly this,—that the phenomenon depends on a portion of the blood, its fibrin, from being liquid, becoming solid, and from being generally diffused in uniform mixture, approximating and collecting in a mass, entangling the red particles and including a portion of the serum.

Now, as generally, during the conversion of liquids into solids, the mere change of form is connected with a disengagement of heat, it might, perhaps, *à priori*, be expected, that a disengagement of heat would be witnessed during the occurrence of the phenomenon in question.

Fourcroy has stated, that the effect of the blood's coagulation is attended with an elevation of several degrees of the thermometer.[1] Mr. Hunter, on the contrary, from his own experiments, inferred that no heat is evolved on the occasion. The experiments, from which he deduced his conclusion, "that in the coagulation of blood no heat is formed," were made on the blood of a comparatively cold-blooded animal, the turtle.[2] Notwithstanding the high authority of the inquirer, and the very favourable circumstances under which his observations were made, his experiments have not been considered entirely conclusive. Dr. Thomson and the late Dr. Gordon, from their own observations, were averse from his opinion, and rather favoured that of Fourcroy—induced to do so, as stated by Dr. Gordon, in his Lectures on Physiology, from witnessing the process of cooling of the blood either retarded or quite interrupted at the time of coagulation.

The consideration of these discrepancies induced me to enter on the inquiry; and I shall now describe some of the experiments made in carrying it on.

The subject of the first trials was the mixed venous and arterial blood of lambs. It was received from the divided vessels into a thin glass bottle of the capacity of eight and a half ounces of water. The bottle was quickly filled, and when full, closed with a perforated cork holding the projecting stem of a delicate thermometer, the bulb of which reached the middle of the vial.

Experiment 1.

The temperature of the blood the instant it was drawn   -   104°
After 1 minute ⎱ coagulation beginning   103·5
    „ 2   „ ⎰   -   102·5

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[1] Ann. de Chimie, vii. p. 147.   [2] Treatise on the Blood, &c. p. 28.

After 3 minutes	-	-	-	-	-	102·5
,, 4 ,,	-	-	-	-	-	102·5
,, 5 ,,	-	-	-	-	-	102°
,, 6 ,,	-	-	-	-	-	101·75
,, 7 ,,	-	-	-	-	-	101·5
,, 8 ,,	-	-	-	-	-	101°
,, 9 ,,	-	-	-	-	-	100·5
,, 10 ,,	-	-	-	-	-	100°
,, 15 ,,	-	-	-	-	-	97·5
,, 20 ,,	-	-	-	-	-	95·0

This experiment was made in the open air in fine weather in summer, in the neighbourhood of Edinburgh, when the thermometer in the shade was 67°. The following experiment was made at the same time: it differed principally from the preceding in two circumstances, viz. in the vial being left open, and in the bulb of the thermometer being near the surface of the fluid.

Experiment 2.

Temperature of blood when drawn	-	-	-	103°
After 1 minute	-	-	-	-	-	103°
$\genfrac{}{}{0pt}{}{2}{3}$ } coagulation commencing	-	-	-	103°
4 minutes	-	-	-	-	-	103°
5 ,,	-	-	-	-	-	102·75
6 ,,	-	-	-	-	-	102·5
7 ,,	-	-	-	-	-	102°
12 ,,	-	-	-	-	-	99°
17 ,,	-	-	-	-	-	96·5

At first view, the temporary pause in the cooling of the blood in these two experiments, just after the commencement of coagulation, would seem to indicate an evolution of heat, commensurate with the arrested cooling effect: but, as the result may be owing to other causes, this conclusion, however specious, may be erroneous. It appeared to me, therefore, advisable to repeat the above experiments on cooling, as accurately as possible, substituting water for the blood. I thus reasoned: if the results in regard to the rate of cooling of the blood and water should be similar, the thermometer being stationary for a time, must depend on some other cause than coagulation; on the contrary, if the fall of the thermometer in the cooling water should be regular and uninterrupted, the pause in the instance of the blood might fairly be attributed to and accounted for by a certain degree of heat evolved.

Experiment 1.

| Temperature of water on admission | - | - | - | 102° |
| After 1 minute | - | - | - | - | 102° |

After 2 minutes	-	-	-	-	-	-	-	102°
" 3 "	-	-	-	-	-	-	-	101·5
·" 4 "	-	-	-	-	-	-	-	101°
" 9 "	-	-	-	-	-	-	-	99°

Experiment 2.

Temperature of water	-	-	-	-	-	106·5
After 1 minute	-	-	-	-	-	106·5
" 2 "	-	-	-	-	-	-	106·25
" 3 "	-	-	-	-	-	-	105·75
" 4 "	-	-	-	-	-	-	105·25
" 5 "	-	-	-	-	-	-	104·75

Experiment 3.

The bulb of the thermometer immersed in the warm water in the middle of the bottle being at 100°, was suddenly thrust to the bottom; it immediately rose to 102°.

These comparative results are manifestly not in favour of any heat being evolved during the coagulation of the blood; from them, it appears most probable, that the pause in the descent of the thermometer is rather owing to a disengagement of heat, from the warmer bottom of the vessel and the deeper fluid.

With the hope of removing all doubt on the point at issue, the following experiment was instituted. To guard against rapid cooling, chiefly the effect of exposure to the air, the bottle for receiving the blood was enveloped in a large quantity of fine wool, which is one of the worst conductors of heat; a thermometer, as before, was introduced into the middle, through a cork, and the vessel was filled with warm water of the temperature 108°. Thus prepared, the carotid artery of a lamb was divided; the bottle was emptied of water, and as soon as possible filled with arterial blood. The thermometer immediately stood at 104·5; it was thus stationary during five minutes; and even after twelve minutes it had fallen only half a degree.

From these results generally, it appears to me, the conclusion is unavoidable, that little or no heat is evolved from the blood, as the effect of coagulation. If it be asked, is this an exception to the general rule, before announced, I would answer that it is not. It was before observed, that what is commonly called the coagulation of the blood, is merely the coagulation of one of the ingredients of this fluid, viz. the fibrin, which constitutes only a very small part of the whole mass; for instance, in the blood of the lamb, it does not exceed 2 per cent. as I have ascertained by experiment. Moreover, it is worthy of remark, that though the coagulation commences suddenly, and is apparently rapid, yet the contraction or condensation depending on the coalescing of the solid particles, is slow;

requiring many hours for its completion. All these circumstances considered, even admitting the law referred to, how small should be the degree of heat evolved in accordance with it, during the act of coagulation! So small, indeed, that it can hardly be admitted to be cognisable by our senses; its detection, I must confess, would appear to me almost miraculous.

In confirmation of this conclusion, I shall give some additional facts on the subject, which I had an opportunity of collecting in 1816, on my voyage to Ceylon, in experimenting on the blood of the turtle and shark.

On the 15th of March, when our ship was in latitude 4° 9′ N. and longitude 19° 15′ W. by chronometer, at sunset, a large shark was taken by means of a harpoon. As soon as it was brought on board, while it was yet alive, it was cut in two. The blood flowing from the great dorsal vein was 82°; the surrounding thick muscles were 82·5, the water of the sea was 80·5, and the air 79°. Some of the blood was collected in a glass vessel. In about two minutes it had firmly coagulated. During the whole time I watched the thermometer immersed in it. The mercury sunk from 81·5 to 81°; it did not rise at the instant that the coagulation commenced, nor did it remain stationary whilst the coagulation was going on, but continued gradually sinking.

The day following another shark was taken. The same experiment was made with the blood, and a similar result was obtained.

On the 23d of March, when in latitude 2° 29′ S. and in longitude 24° 30′ W. a large turtle was killed, which had been caught about three weeks before at the island of Ascension. The air at the time was 79°; the blood of the turtle, flowing from the carotids, was 91°. When collected in a tumbler it was 88·5. The thermometer placed in the midst of it immediately began to fall, and continued falling gradually, without any sensible interruption, whilst the blood was coagulating.[1]

[1] On this subject I made, in Ceylon and Malta, some additional experiments. Bearing date of Colombo, May 4th, 1817, is the following note, which I shall transcribe verbatim. "Testudo Mydas—about 50 lbs. weight—caught yesterday; thermometer in recto 84·5; stream of arterial blood flowing from the divided carotids 85°; temperature of air 86°.

"Made three experiments with the blood to ascertain, under such favourable circumstances, whether any heat is evolved during the coagulation of the blood. The general results proved decidedly that there was no sensible heat evolved.

"The blood, as it flowed, was 85°; collected in a glass vessel it was the same; it coagulated in about a minute,—still no change; in about two minutes the coagulum was firm,—temperature still the same. In about half an hour the blood had acquired the temperature of the air, which was then 87°."

And another, bearing date of the 23d October, 1819.—"A large green turtle was brought to me this morning. The great vessels of the neck were immediately divided. A wine-glass was quickly filled with arterial blood: a thermometer in it rose to 82° (air at the time 79°); in two minutes it was

These results were inserted in a paper published in the second volume of the Journal of the Royal Institution, with some others, in reply to Dr. Gordon. This gentleman, about eighteen months previously, had endeavoured to support his original opinion by further experiments, an account of which appeared in Dr. Thompson's Annals of Philosophy. I shall add my remarks on them, believing that they may help to explain the effect, in accordance with what I witnessed.

The difference between the results of Dr. Gordon's experiments and my own arose, perhaps, from the different modes in which our experiments were made. Dr. Gordon, I believe, kept the bulb of his thermometer near the bottom of the vessel containing the blood— and when this fluid began to coagulate towards the surface, he drew the instrument up. On the contrary, in all my latter experiments, viz. those on the blood of the shark and turtle, and many which were made on the blood of sheep at the Cape of Good Hope, the thermometer was not allowed to remain stationary—it was gently moved from one part to another, so that the whole might be kept of the same temperature, till the coagulation commenced ;—for when the blood is viscid and the vessel deep, the surface or the portion last drawn is warmer than that below—and when shallow, the bottom is warmest, as I have frequently observed in my experiments.

Further, in confirmation of the conclusion that no appreciable degree of heat is noticed during the coagulation of blood—I may mention, analogically, the fact, that when the serum of the blood is coagulated by means of dilute nitric acid, there is not the slightest elevation or change of temperature. The following are the particulars of an experiment in proof of this. An ounce of serum was added to an ounce of dilute nitric acid, each of the temperature of the air of the room, viz. 75°; coagulation was immediately produced, but not the slightest change of temperature was occasioned ; the mixture was exactly 75°. The dilute nitric acid used contained five parts of water to one of strong acid. The serum employed was of specific gravity 1·030; and when slowly evaporated to dryness, an ounce of it afforded a brittle translucent residue, weighing 36·7 grains. It was procured from the blood of a patient labouring under a tedious chronic disease; which blood was only of specific gravity 1·047; and an ounce of it afforded 2·4 grains of dry fibrin.

coagulated slightly—thermometer still 82°; in five minutes it had coagulated firmly—thermometer 81°. A very delicate thermometer was used."

At Malta, a similar trial was made on the 21st December, 1838, on the blood of a turtle, which had been kept about two months in water, without eating, and yet was active. The temperature of the water was 58°, and that of the air of the room 60°, two hours earlier it was 58°; the blood of the turtle, flowing from the great vessels of the neck (the jugular veins and carotid arteries) was 58°. It was collected in cups, and, in about half an hour, was firmly coagulated. During this time, whilst the coagulation was taking place, I tried, repeatedly, the temperature of the different portions with a delicate thermometer, but could perceive no change.

The inference from this fact, reasoning analogically, seems to me unavoidable, viz. that it is highly improbable any heat can be given off, during the coagulation of the blood, in which so small a quantity of matter becomes solid, and that slowly—since not the smallest degree is evolved during the coagulation of serum, when so large a quantity of matter, so similar to fibrin in its nature, is solidified, and that suddenly.

I would remark, in conclusion, that as regards the general rule before referred to, of change of temperature with change of form—it has so many exceptions, that no reliance can be placed on it. Every instance requires to be specially considered and determined experimentally. Thus, contrary to rule, theoretically considered, all the detonating compounds, whether they contain oxygen or not, emit heat in the act of explosion, and of conversion entirely, or in part, into the state of an elastic fluid; thus, carbonic acid, when expelled from lime by dilute muriatic acid, is converted from the solid to the gaseous state, without any change of temperature; and thus, further, when ammoniacal gas and muriatic acid gas combine, they form a solid salt, without the slightest disengagement of heat.

2.—*Observations to determine whether there is any well marked difference, in point of time, in the coagulation of venous and arterial blood, and in the degree of contraction accompanying it.*

To endeavour to determine the first question, the following experiments were made in Edinburgh, during the summer of 1811, on the blood of lambs. The two kinds of blood compared were taken from the same animal—one from the jugular vein, the other from the carotid artery; and each was received in a glass vessel, either the same or similar, of about three ounce capacity. The thermometer in the open air, where the experiments were made, stood at 70°.

1. Venous blood;—in four minutes there was a commencement of coagulation; in six it was firmly coagulated.

Arterial blood;—in half a minute it had begun to coagulate; in two minutes it was firmly coagulated; and in three, there was a distinct separation of serum.

2. Venous blood;—in two minutes there was a commencement of coagulation, and in two and a half, it was firmly coagulated.

Arterial blood;—its coagulation commenced as it flowed from the vessel; in a minute it was firmly coagulated.

3. Venous blood;—in two minutes it was slightly coagulated.

Arterial blood;—in one minute it was firmly coagulated.

From these experiments and others, the results of which were similar, and which, therefore, it is unnecessary to describe, it appears that arterial blood coagulates with greater rapidity than venous.

Relative to the degree of contraction attending the coagulation of each kind of blood—it was unquestionably greatest in the instance of arterial; the difference of appearance was so distinct, measured merely by the eye, that there could be no mistake.   In every comparative trial, after many hours, the venous blood was found softer and less contracted.

3.—*Observations on the degree of coagulability of the first and last portion of blood from a slaughtered animal.*

Mr. Hewson observed that blood which flows last from a slaughtered animal coagulates more readily than that which flows first.[1] Mr. Hay, in his work on the blood, inferred the contrary from his own experiments.[2]   The trials which I have instituted on this point, using the blood of the lamb and sheep, perfectly accord with those of Hewson.   I shall give a few of the results.   The same glass vessels were employed to collect the blood in as before.   The temperature of the air was 68°.   In one vessel, the blood which first gushed out, on the division of the great cervical vessels, was caught—and, in another similar, that which flowed just before the animal expired.

1. On the sheep.—The blood which flowed first was firmly coagulated in two minutes; but that which flowed last, in half a minute.

2, 3, and 4.—In three experiments on lambs, the results were perfectly similar; the first portion coagulated in one minute; the last in half a minute.

It is not easy to reconcile these results, and those of Hewson, with Mr. Hay's.   His, perhaps, may be held as exceptions.   The two former at least show that in certain cases, the blood which flows from an exhausted animal possesses an unusual proclivity to coagulate: and if consequences are attended to, it is difficult to avoid considering it, whatever may be its cause, as a happy provision of nature to aid in arresting hemorrhage.

4.—*Observations on the specific gravity of the different parts of the blood—viz. the serum, coagulable lymph, and red particles.*

Mr. Hunter, from a few very simple experiments, arrived at the conclusion, that of the different constituent parts of the blood, or those in which it is resolvable when abstracted from the vessels, the red particles are of the greatest specific gravity, the serum of least, and the coagulable lymph intermediate.   That the red particles are as he considers them, is now universally admitted : but it has been more than doubted by one inquirer, that the serum is

[1] Hewson on the Blood, p. 62.   [2] Hay on the Blood, p. 26.

lighter than the lymph;[1] and by another, that there is any difference of specific gravity between them.[2]

I have repeated the experiments, on the results of which Mr. Hunter founded his conclusion, and I have no hesitation in adopting it entirely. In every instance in which, according to his method, I have immersed coagulable lymph in serum, I have invariably seen it sink, unless buoyed up by adhering particles of air.

The specific gravity of the different parts of the blood is probably variable within certain limits, as is the specific gravity of the blood itself, even within the limits of health, according to a variety of circumstances, which it may be difficult to enumerate, and much more to appreciate, connected with age, sex, climate, diet, &c.

I shall not in this place endeavour to determine what may be considered the standard specific gravity, either of the blood itself, or of its serous or fibrinous parts. To accomplish this, a very wide comparison of accurate observations would be required, beyond my reach; and which, I fear, in the present state of our knowledge, is hardly practicable.

From the results of the experiments which I have had an opportunity of making on healthy serum, and which will be individually given hereafter, I may state that I have found it to vary between 1020, and 1031.

The trials which I have made of the specific gravity of coagulable lymph or fibrin have been comparatively few in number. I can find mention of three only in my note-books. In the 1st, it was found of specific gravity 1046; in the 2d of 1057; and in the 3d, 1060. The first was procured from blood not buffed; was separated by washing with water, and the fibrin or lymph thus obtained, was weighed after having been gently pressed between folds of blotting paper. The second was a fibrinous concretion from the heart, free from colouring matter; it was weighed without having been subjected to the action of water, after having been gently pressed in blotting paper. The third was a portion of buffy coat; it was weighed after having been well washed with water, and the excess of moisture removed, as in the two preceding instances, by paper.

My experiments on the specific gravity of the red particles have also been few in number. From those on the results of which I think most reliance can be placed, it would appear, that the specific gravity of a red particle is about 1132, which very nearly agrees with the estimate of Jurin, indirectly obtained, made more than a century ago.[3] The method by which I arrived at this result, was by putting a minute portion of blood, less than a drop, in solutions of different degrees of strength of sulphate of soda. In one of specific gravity 1090, the red particles slowly subsided: in that of

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[1] Sir C. Scudamore. Essay on the Blood, p. 35.

[2] Dr. Benjamin Babbage, Med. Ch. Trans. of London, vol. xvi. Part II. p. 304.

[3] He estimated it at 1126. Phil. Trans. for 1719.

the specific gravity above mentioned, when agitated in it, they remained for some time suspended, but rather inclining downwards than upwards. This was nearly a saturated solution at a temperature of about 120°: it ultimately deposited a considerable quantity of crystals; but it remained without change sufficiently long to allow of the trial described being made. This result is perhaps more deserving of confidence in as much as the red particles did not appear to be materially changed by the strong saline solution; under the microscope they exhibited their usual disc-like form. .

Relative to the cruor, or admixture of the red particles and serum, I may mention one or two trials which I have made on its specific gravity, weighing it in the usual manner. A mass of crassamentum from blood, the specific gravity of the whole of which was 1055, and the serum of which was 1029, was broken up and subjected to moderate pressure in a linen bag without any addition of water. The liquid which passed through, consisting of red particles and a little serum, was found to be of specific gravity 1074. This cruor was next poured on a filter of cotton cloth, to drain off as much as possible of the serum. It was left for twenty-four hours covered over with a glass receiver to prevent loss by evaporation. Now weighed again, it was found of the increased specific gravity 1087. It was of the consistence of molasses; and was not further concentrated by being kept in a filter of very bibulous paper : the paper became saturated with it; but not a single drop separated.

Probably the phenomena of the buffy coat have given rise to the idea, before alluded to, in opposition to Hunter, of the comparative lightness of fibrin. The buffy coat appears on the surface of the blood, and therefore seems naturally to suggest the idea. But when the circumstances are inquired into, under which it forms, there is no difficulty in relinquishing it.

In sizy blood, or that blood which affords a buffy coat, the serum and coagulable lymph appear to be of greater tenuity or less viscid than in blood that is not sizy; and sizy blood is generally slower in coagulating than blood that is not sizy, although not universally so. When the former quality alone prevails, the buffy coat which forms is thin; when both qualities coexist, the buffy coat is thick, from the greater depth of the subsidence of the heavier red particles. Now, though the fibrin of the buffy coat is of greater specific gravity than the serum,—the *liquid* mixture of coagulable lymph and of serum,—the liquor sanguinis, as it has recently been called, —is of less specific gravity than the liquid mixture of red particles, serum, and coagulable lymph, and in consequence the former becomes supernatant. Before coagulation takes place, it is this comparatively dense fluid which supports the liquid fibrin, and when coagulation has occurred the substratum of red particles enveloped in fibrin still gives support; and, the longer the coagulation is in taking place, so much the less fibrin is mixed with the red particles, so much the more rises or is pressed up, and, *cæteris*

paribus, so much the thicker the buffy coat becomes. In healthy blood, on the contrary, in which the serum and coagulable lymph are more viscid, the red particles are supported till coagulation takes place, and then are retained, like water in a sponge, and, like water, may be expressed from the sponge-like texture; or, if in excess, a part of them will separate and fall down as the fibrin contracts. This view is in accordance with Hunter's well authenticated facts, confirmed as above. And, as another experimental confirmation of them, I may mention, that when fibrin, or a portion of buffy coat which sinks in serum, is plunged into the cruor of blood, it rises to the surface and floats, in accordance with their respective specific gravities.

Relative to the second opinion, already referred to, that there is no difference of specific gravity between serum and lymph, it appears to me inadmissible, simply because it is not in accordance with facts. The ingenious author of it seems to found the idea chiefly on the circumstance of the uniformity of composition of the crassamentum in relation to fibrin. Granted that the crassamentum is uniform in this respect, that there is no accumulation of fibrin towards its bottom, and no deficiency of it towards the surface, it does not therefore follow that the fibrinous part, not even when liquid, as coagulable lymph, is of the same specific gravity as the serum with which it is equally mixed, and much less when solid. It might, it appears to me, as well be said, that the serum of the blood and water is each of the same specific gravity, because they are miscible, and that equally. The mere circumstance of the coagulable lymph and serum being in intimate mixture, appears to me to be quite sufficient to account for the fact of the uniform composition of the crassamentum, admitting it to be so.

Dr. Benjamin Babington has connected the above idea with some views peculiar to himself, relative to the liquid part of the blood, which I must confess do not appear to me to be well borne out by the facts. The opinion of Hewson and of Hunter appears to me more in harmony with them. They considered the fluid part of the blood an equable mixture of liquid lymph, and of serum; both capable of being separated from the mass of the blood, apart, or together, under peculiar circumstances in health and disease; the one spontaneously coagulable, the other not. Dr. Benjamin Babington, on the contrary, is of opinion, that no such fluid as serum exists in the blood, and no such fluid as coagulable lymph; but a liquor sanguinis, not capable, compatibly with health, of affording either serum alone, or lymph alone, and capable of being separated into serum and lymph or fibrin, only when removed from the circulation, and not under the influence of the laws of life. And in accordance with this view he considers the denomination of serous membrane improper.

If serum was never found in closed cavities, without its due proportion of lymph, (taking as a measure the albuminous part of each,) then this view would be plausible; but as this is not the case

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—as serum, differing very little in specific gravity from the serum of the blood, is not unfrequently to be found in such cavities, especially in the pericardium, unaccompanied by any lymph, (as I have often satisfied myself,) or by the deposition of lymph in the form of a false membrane, the hypothesis appears to me untenable. Dr. Babington admits the facts I have alluded to in the latter part of his paper, where, contrary to what he advances in the first part of it, he has recourse, to explain them, to the ordinary doctrine of secretion.

5.—*Observations on the specific gravity of blood, venous and arterial, of different animals, and of different ages, not including man.*

As a preliminary, it may be right to mention the method employed in the trials, the results of which are about to be given. It was the same as that now commonly used for ascertaining the specific gravity of other fluids. On entering upon the inquiry, I provided myself with a certain number of thin globular bottles, varying in capacity from 200 to 400 grains of water. Each bottle had a narrow neck, to which was adapted by grinding a perforated glass-stopper, so as to admit of accuracy in filling. The weight of the fluid examined was always compared with that of distilled water; and trial of the latter was made as often as any material change of temperature occurred to render it advisable. In each instance, before making trial of the blood, it was allowed to acquire the temperature of the atmosphere, and then the small vacant space in the bottle, the effect of cooling, was filled with serum. In the earliest experiments a balance was used of no great delicacy: it was affected, however, readily by the one tenth of a grain. In the latter, one was substituted which was affected by the one fiftieth of a grain: and in the latest of all, one still more delicate, constructed by Robinson, which when loaded with 600 grains, was turned by an hundredth of a grain.

For the sake of brevity, I shall give the results of my experiments in a tabular form.

Table I.

Showing the specific gravity of blood, venous and arterial, of different animals, and of different ages, not including man.

Animal.		Age.	SPECIFIC GRAVITY.					Place.	Season.
			Arterial blood.	Venous blood.	Arterial serum.	Venous serum.	Mixed venous and arterial blood.		
Sheep	1	—	1050	1056	1025	1027	—	Edinburgh.	Summer.
"	2	6 years.	1057	1058	1030	1030	—	"	"
"	3	16 mths.	1049	1051	—	—	—	"	"
"	4	"	1047	1050	—	—*	.—	"	"
"	5	3 years.	1047	1051	1019	1029	—	Malta.	"
Lamb	1	11 wks.	1052	1055	1027	1028	—	Edinburgh.	"
"	2	"	1046	1057	1024	1024	—	"	"
"	3	"	1054	1054	1024	1024	—	"	"
"	4	"	1050	1053	1024	1024	—	"	"
"	5	"	1047	1050	—	—	—	"	"
Horse	-	12 years.	—	1053	—	1027	—	Chatham.	Spring.
Zebra	-		—	1053	—	—	—	Edinburgh.	Autumn.
Ox	-		1058	1061	1027	1029	—	"	Summer.
Calf	-		1040	1046	1022	1023	—	"	"
Dog	-		1048	1053	1022	1023	—	"	"
Pig	-		—	1060	—	—	—	"	"
Cat	-		—	1050	—	—	—	Chatham.	Winter.
Turkey	-		1061	—	1021	—	—	"	"
Salmon	-		—	—	—	—	1051	Edinburgh.	Summer.
Holybut	-		—	—	—	—	1028	Chatham.	Spring.
Cod	-		—	—	—	—	1034	"	"
Plaice	-		—	—	—	—	1032	"	"
Skate (R. batis)	-		—	—	—	—	1035	"	"
Dogfish (M. lævis)		—	—	—	—	1022	"	"
Eel (M. latirostris)		—	—	—	—	1042	"	"
Frogs, males	-		—	—	—	—	1040	Edinburgh.	Summer.

In all the instances of the mammalia the blood taken was from the jugular vein and carotid artery.

All the animals were in apparent good health at the time the trials on the blood were made, with the exception of the zebra, which had a tumour in the nostril, and of which it died after a few weeks.

From the results stated in the table, it clearly appears that arterial blood is of somewhat less specific gravity than venous, and arterial serum than venous serum. Taking the mean of all the

* In this trial, the venous blood was first received; the arterial when the vessels were nearly emptied.

experiments, the specific gravity of each kind of blood and serum, appears to be as follows :

Arterial blood	-	-	-	-	1050
" serum	-	-	-		1022
Venous blood	-	-	-	-	1053
" serum	-	-	-		1026

Relative to the cause of the greater specific gravity of venous blood, it probably does not solely depend on the arterial blood's containing a larger proportion of water ; it probably depends partly on this cause and partly on the venous blood containing, besides a smaller proportion of water, a larger proportion of animal matter. But what the kind of matter is, whether the coloured particles, or fibrin, or some other substance, as free carbon, remains to be determined. Independent of this point, there seems to be little difficulty in accounting for the difference, considering the many fluids which are secreted from the arterial blood, and those comparatively dilute, and thereby manifestly tending to augment the density of the venous blood. It was long ago observed by Dr. Crawford, that arterial blood is apparently more liquid than venous ; if so, in reality, which is probable, it may be partly owing to its being more dilute, as indicated by its specific gravity.

The number of experiments on the specific gravity of the blood and serum of animals of different ages is hardly sufficiently large to allow of any satisfactory conclusion ; they seem to indicate that the blood of adult animals is of greater specific gravity than that of young—an inference in accordance with analogy, and which Bryan Robinson, one of the ablest of the iatro-mathematical sect, many years ago deduced from his own experiments.[1]

That the specific gravity of the blood of different animals varies, is manifest from the preceding table. The difficulty is to reply to the question, in what manner the variation accords with the nature or species of animals.

As the degree of density of the blood appears to depend chiefly on the proportion of red particles; and as the use of the red particles seems to be connected less with nutrition than with action, according to the views of Hunter,[2] and more, I may add, with the production of animal heat, than perhaps with any other function, it is very probable, that the blood of birds is of greatest specific gravity, (varying in degree according to the temperature of each individual, and perhaps its strength and energies,)—that the specific gravity of the blood of the mammalia is next,—next that of reptiles,—then that of fishes,—and lastly, that of the different classes of animals having colourless blood, amongst which it is probable, grounded on the preceding considerations, that the blood of insects will hold the highest place. Many things already known favour

[1] Vide Animal Economy, p. 245.   [2] Hunter on the Blood, p. 431.

this view of the subject : but still it must be considered hypotheti-
cal, until it has been fairly investigated experimentally.

6.—*Observations on the specific gravity of the blood of animals blooded to death.*

To endeavour to determine whether, in the instance of animals
blooded to death, any difference exists in the specific gravity of the
blood which flows first, and that which flows last,—I made a few
experiments in Edinburgh in the summer of 1812, the results of
which are contained in the following table :

TABLE II.

Animal.	SPECIFIC GRAVITY.			
	1st. Arterial blood.	2d. Arterial blood.	1st. Serum.	2d. Serum.
Sheep 1	1049	1048	1024	1023
" 2	1050	1044	1027	1022
Lamb 1	1049	1046	1024	1020
" 2	1051	1045	1024	1018
Ox 1	1058	1051	1027	1021

All these results seem clearly to show, that the blood which flows
last, when the vessels of the animal are nearly exhausted of their
fluid contents, contains a diminished quantity of animal matter, or
an increased quantity of the aqueous part. And that the difference
of specific gravity is mainly owing to the latter, rather than the
former cause, may be inferred from the circumstance, that there is
not a greater difference in the specific gravity of the blood before
the separation of the serum, than in the serum itself. Whence it
may be asked, is this dilution of the blood, this augmentation of its
watery part? Does it depend on increased activity of the absorbent
vessels, in connection with an undue langour of the terminal secern-
ing arteries? or solely on the debility of the latter, the action of the
former remaining neither diminished nor altered? It appears to me,
that the latter supposition is most probable, and is most accordant
with the general doctrines of pathology and therapeutics.[1] I know
not whether the results obtained by Mr. Hay, will bear the same
interpretation. He, from his experiments, inferred that the last
portions of blood which flowed contain less serum than the first, in
the ratio of 28.8 to 55.8.[2] But his results are defective, inasmuch

[1] It may be owing to an unusual flow of the contents of lymphatics into
the circulation, as the blood flows out : the lymph certainly is of much less
specific gravity than the blood,—whether spontaneously coagulable as Hew-
son found it, in different instances in which he examined it, or only coagu-
lable by heat, as I have occasionally found it in the lymphatic vessels of the
spermatic cord of the bull.

[2] Op. cit. p. 28.

as he did not determine the specific gravity of the serum, merely the quantity of it which separated in the act of coagulation. The crassamentum of the blood, as already observed, and as is now generally admitted, consists of red particles, serum and fibrin,—the serum contained or held in the interstices of the fibrin as in a sponge; and consequently, the softer the fibrinous mass is, or the less contracted, so much the more serum it must hold. These remarks apply to the deductions of Mr. Hay: the *argumentum ad absurdum* is also applicable to them; for by adding water to the blood before coagulation, the volume of the coagulum is increased, and the separation of fluid from it is diminished: therefore the inference from the quantity of serum, independent of its specific gravity, is of no weight in this question.

7.—*Observations on the specific gravity of human blood and serum in instances of disease.*

The following observations were made at three different periods, viz. in the years 1811–12, in Edinburgh; in 1818–19, at Candy in the island of Ceylon, and in 1826–27, in Corfu. As both the climate and the circumstances affecting the condition of the individuals were very different, I shall give the results obtained at each period, in a table apart.

In these instances the same precautions were taken in ascertaining the specific gravity of the blood as in the former: and, I may add, that the blood from which the serum was collected for trial, was kept so as to prevent evaporation, either in a bottle and corked, or defended by a cover if in a bleeding-cup. The necessity of this to insure accuracy is obvious: if the air have free access to the surface of the blood, a portion of the aqueous part will necessarily be carried off by evaporation, and the specific gravity of the serum will be augmented.

1.— *Table showing the specific gravity of blood and serum in instances of disease in Edinburgh.*

TABLE III.

Age and Sex.	Disease.	Sp. Gr. of Blood.	Sp. Gr. of Serum.
WOMEN.			
30 years	Phthisis pulmonalis: the blood not buffed: little pyrexia	1054	1027
25 "	Mild pneumonia		1023
24 "	Severe catarrh, with much pyrexia	1061	1028
22 "	Pleuritus: the blood puffed	1058	1026
26 "	Severe headach: slight pain of side: slight pyrexia	1056	1031
40 "	Hemiplegia		1026
27 "	Emansio mensium	10518	1028
Ditto	Convalescent		1029
17 "	Chlorosis.—Serum green	1055	1027
60 "	Slight cough, and pain of side, without pyrexia	1059	1026
MEN.			
20 years	Severe headach, the effect of high temperature; without pyrexia	1062	1026
33 "	Severe headach during convalescence from fever	1061	1027
23 "	Pneumonia: blood slightly buffed	1060	1025
23 " Negro	Slight pneumonia	1061	1033
19 years	Phthisis pulmonalis; a short time before death	1045	
42 "	Diabetes mellitus	1061	1030
19 "	Diabetes mellitus		
	Blood drawn at evening	1060	1025
	Blood drawn in the morning	1062	1026
	Blood drawn at evening with milky serum	1058	1026
	Blood drawn in the morning not milky	1050	1023

The blood in each instance was venous, and was taken from the arm. The individuals from whom it was abstracted, were patients in the Royal Infirmary, and belonged chiefly to the working class, and were, with few exceptions, inhabitants of the town.

The results are too limited in number to admit of any general conclusions being deduced from them in a clear and satisfactory manner. The following inferences, however, they seem to warrant, as probable:—viz. that in inflammatory complaints the proportion of animal matter is increased, the proportion of water diminished; and also that the density of the female blood is less than that of man. The former inference accords with the experiments of Robinson, Tabor, and Langrish, which, although imperfect in themselves, may at least perhaps be depended on for comparison. In the majority of the examples adduced, nothing unusual was manifest in the appearance of the blood. Whenever the blood exhibited a

buffy coat, the inferior surface of the mass of crassamentum appeared softer than usual, as if from a deficiency of fibrin ; which is not surprising, considering that the specific gravity of the red particles is greater than that of the fibrin. The green colour of the serum of the chlorotic woman is worthy of remark ;—its cause, I was not able to ascertain ; it did not appear to depend on the pre- sence of bile. The blood of the man labouring under diabetes was, in each instance, less firmly coagulated than healthy blood. In two instances, the serum of this blood, last drawn, had a milky hue : a common occurrence in this disease, and which I first learned from the lectures of Dr. Home. The serum of this blood I fre- quently examined according to Dr. Wollaston's method, but unsuc- cessfully ; I could never detect in it a particle of saccharine matter : nevertheless, I believe it contained an undue proportion of animal matter : this was indicated by its comparatively high specific gravity. In the last case of diabetes, the depleting plan of treatment was fairly tried : blood was abstracted for some time every other day, and the effect in diminishing the specific gravity of the blood and the serum was well marked. In this case, further, it may be mentioned that some relation appeared between the blood and the urine. The specific gravity of the urine was always greatest in the morning ; when it uniformly exceeded 1040, and was once as high as 1051 ; and yet in other respects, it then appeared most analogous to healthy urine ; it abounded in urea, and contained hardly a trace of saccharine matter. In the evening, on the con- trary, when its specific gravity was least, viz. between 1030 and 1040, the urea gave place to saccharine matter : the former could not be detected in it ; the latter abounded. And, like the urine, the blood in this case was of greater specific gravity in the morning than in the evening.

2.—*Table showing the specific gravity of blood and serum, and also of bile in certain instances of disease, in the interior of Ceylon.*

TABLE IV.

Age.	Disease.	Blood	Serum.	Bile.
25	Fatal remittent fever; no organic disease could be detected by dissection; the liver appeared healthy; it weighed 4lb. 10oz.; about an ounce of glairy yellow bile in gall-bladder	—	—	1011
27	Convalescent from a severe attack of remittent fever; seized with pain of side	1054	—	—
23	Fatal remittent fever; about 2oz. of brownish bile in the gall-bladder; no well-marked lesion dis- covered	—	—	10133
35	Remittent fever (2d day); blood not buffy; serum of the colour of ale	10541	10238	—
	24 hours after, after having been well purged, was blooded again on account of severe headach	—	10240	—

Table continued.

Age.	Disease.	Blood.	Serum.	Bile
42	Slight remittent fever (3d day); blood not buffy; serum tinged slightly red	10585	10281	—
30	Intermittent fever; 8oz. of blood abstracted in hot stage, which it cut short; blood of natural appearance; serum of reddish hue	10588	10281	—
31	Fatal remittent fever, with yellowness of skin, and bloody urine; the inner coat of bladder of urine, of stomach, and duodenum, red, as if inflamed; liver soft; gall-bladder turgid with dark, thick, ropy bile; mouth of common ducts apparently inflamed, but not obstructed	—	—	10556
29	Continued fever (3d day); second V. S. not buffed; very dark and soft	10551	10281	—
40	Remittent fever (2d day); it began as intermittent, on a severe march; pain in hypochondria; buffy coat of blood near half an inch thick; serum light yellow	—	10290	—
40	Remittent fever with pain of chest (7th day); very slight buffy coat	—	10250	—
35	Ephemeral fever, after confinement in hospital for a peculiar debility of legs; no buffy coat; crassamentum soft; serum limpid and red	—	10279	—
25	Fatal remittent fever; liver weighed 5lb 4oz.; bile dark and ropy	—	—	10445
35	Continued fever (6th day); blood not buffed; serum very yellow	—	1022	—
35	Slight pneumonia; blood not buffed; serum of reddish tinge .	1055	1027	—
35	Severe catarrh; some pain of chest; no buffy coat on blood; serum of reddish hue	—	10295	—
30	Negro:—Slight pneumonia; blood not buffed; serum of ale colour	—	10296	—
	2d day; aggravation of complaint; V. S. repeated; collected in two portions; first portion not buffed; of natural appearance .	—	10296	—
	2d portion, a good deal buffed and cupped	—	10275	—
30	Sepoy:—Pneumonia; a week ill; blood of natural appearance, collected in two portions; serum of the colour of porter; first	—	10286	—
	Second .	—	10281	—
40	Epilepsy, with severe headach .	—	10295	—
25	Hepatitis, with occasional dysenteric symptoms; 2d V. S. Blood collected in two portions; first portion slightly buffed; second, of natural appearance; both of .	—	10274	—
27	Chronic hepatitis; blood not buffed; serum greenish yellow	—	10281	—
30	Hepatitis; blood not buffed; serum of the colour of ale, after a severe and harassing march	—	10316	—
30	Acute dysentery (10th day); 2d V. S. Blood very slightly buffed and cupped	—	10288	—
	3d V. S. after half an hour; blood even less altered .	—	10278	—

TABLE continued.

Age.	Disease.	Blood.	Serum.	Bile.
30	Acute dysentery; blood not buffed; serum slightly tinged red; after a severe and harassing march*	—	10303	—
30	Acute dysentery; blood not buffed; serum light greenish yellow	10565	10281	—
	Died on 6th day. Rectum and colon gangrenous. Bile of natural colour, ropy	—	—	10371
31	Acute dysentery, fatal on the 10th day; inflammation, thickening and ulceration of large intestines; inflammation of peritoneum and of small intestines; gall-bladder distended with dark ropy bile	—	—	10491
28	A notorious drunkard; died of dysentery and abscess of liver; large intestines inflamed, thickened and ulcerated; five small abscesses in the liver, containing a puriform fluid; the gall-bladder distended with viscid bile, not distinctly morbid; the liver weighed 4lb. 13oz., and was softer than natural	—	—	10113
25	Dysentery, after ague; for some time they alternated; blood not buffed; serum of colour of Madeira wine; it was collected in two portions; first portion	10590	10330	—
	Second portion	—	10316	—
30	Fatal dysentery; colon and rectum thickened and ulcerated; ileum inflamed; bile of natural appearance	—	—	10165
28	Fatal dysentery; cœcum ulcerated: colon ulcerated and gangrenous; ileum slightly inflamed; in right lobe of liver two abscesses, one holding about 8oz. of matter, the other about 2oz.; dark brownish bile in gall-bladder destitute of mucus	—	—	10430
27	Dysentery, after intermittent fever; blood of natural appearance	—	10215	—
30	Fatal dysentery, complicated with abscess and gangrene of liver, not suspected during life; large intestines thickened and ulcerated; about three inches of lower portion of ileum severely ulcerated; a large cavity in the liver, its walls gangrenous, full of a fœtid bloody fluid; gall bladder distended with brown bile . . .	—	—	10238
27	Fatal dysentery; liver of natural appearance; gall-bladder distended with thin, limpid, green bile	—	—	10107

* This man was one of a chosen party of about sixty, who in twenty-one hours made a most severe march from Atgalé to Nalandi, and after sleeping on the ground two or three hours, returned in about the same time. They were sent to relieve a post which they found evacuated. Great part of the way, both in coming and going, they were opposed by the enemy, and found the road variously obstructed. Their exertions were extreme; every man of the party, with the exception of the commanding officer, was in the sick-list within the last fortnight, labouring under remittent fever, dysentery, and hepatitis, and a considerable proportion of them died.

TABLE continued.

Age.	Disease.	Blood.	Serum.	Bile.
27	Gun-shot wound in lumbar region; V. S. on 3d day; blood received in two portions; first, slightly buffed	—	10279	—
	Second, free from buff	—	10278	—
23	Gun-shot wound of lung. V. S. 4 hours after; blood of natural appearance	—	10295	—
	After about 10 hours V. S. repeated, when labouring under much dyspnœa, with pyrexia; blood not buffed	—	10275	—
20	Malay:—Gun-shot wound of lung. V. S. on 3d day; blood not buffed	—	10325	—
30	Singalese:—Gun-shot wound of neck, implicating trachea and œsophagus. V. S. on 3d day; blood not buffed; serum of the colour of porter .	—	10303	—
22	Singalese:—Gun-shot wound of back. V. S. 6 hours after; blood of natural appearance .	10508	—	—
30	Fatal gun-shot wound of pelvis and spine; the hepatic duct in this instance exhibited an unusual peculiarity; just within the substance of the liver it was distended into a cyst, into which several branches opened, and from which a duct passed to the gall-bladder; it contained a good deal of thick, viscid, greenish mucus; there was healthy bile in the gall-bladder	—	—	10165
20	Negro:—Contusion of side from a fall; blood not buffed; serum reddish	—	10281	—
20	Singalese:—Contusion from a fall; blood not buffed	1058	10316	—
28	Fatal tetanus, after gun-shot wound fracturing both bones of fore-arm; tetanus began on 2d day; amputation was performed on the 3d, without benefit; death on the 5th; blood coagulated, not buffed; bile dark brown	—	—	1043

The observations contained in this second table were all made in the military hospital in Candy, during the rebellion in 1818–19, when our troops were most actively employed in the field, were exposed to excessive fatigue and severe privations, often in most unhealthy parts of the country, and suffered dreadfully from disease, especially from those scourges of camps, remittent fever and dysentery. Some idea of the severity of the diseases may be formed from the fact, that the mortality from disease alone, whilst the rebellion lasted, was about twenty-five per cent. of the strength. Besides the specific gravity of the blood or serum, in many instances I ascertained that of the bile; which I have thought it right to give, although not strictly belonging to the present subject.

All the cases referred to, with the exception of the Singalese, who were camp followers employed as coolies or porters, were men belonging to our regiments, serving in Ceylon, the Negroes and Malays to the 1st Ceylon regiment, the others to the 19th, 73d, and

83d foot. The Negroes were Africans, of Mozambique; the Malays were natives of Java, or the adjoining islands; the men belonging to the other regiments were either English, Scots, or Irish.

As regards the general scope of the results, some plausible inferences perhaps might be drawn from them, in relation not only to the effects of disease on the circulating fluids, but also on the influence of climate, variety of race, &c. But I think it best to abstain from the attempt, considering the basis of facts as too slender for a foundation for theoretical views. As facts, however, bearing on an important part of pathology, few and imperfect as they are, I have thought it right to bring them forward, even after the lapse of twenty years, with the hope that they may not be altogether useless in themselves, and that they may lead to further inquiry.

3.— *Table showing the specific gravity of blood and serum in instances of disease in Corfu, and also the proportion of fibrin.*

TABLE V.

No.	Age.	Disease.	Blood.	Serum.	Fibrin per 1000 parts.
1	20	Remittent fever; blood of natural appearance	1060	—	1.7
2	22	Remittent fever; blood slightly buffed	1052	—	5.25
3	25	Chronic visceral disease of many months' duration. V. S. repeatedly performed	1047	1030	5.
4	23	Slight erysipelatous inflammation of arm; blood slightly buffed, and the first portion more than the second	1054	1025	3.76
			1053	—	3.4
5	26	Epileptic fit preceded by rigour. V. S. to 24oz. First portion had a pretty strong buffy coat; the last in a less degree	1044	—	4.78
			1042	1023	3.7
		The following day V. S. repeated; 2lbs. abstracted, not buffed; the last portion	1032	1021	4.1
6	25	Slight pain of chest without pyrexia after intoxication. V. S. to 3lb.; blood slightly sizy; no distinct difference between first and last portion	1055	1029	1.3
			1054	1027	1.3
7	20	Obscure abdominal disease (tumour) after fever. V. S. to 24oz.; first portion slightly buffy; second portion in a less degree	1054	1027	1.7
			1052	—	1.6
8	20	Sudden pain in region of liver with pyrexia when convalescent from fever. V. S. to 8 ounces, syncope. Blood buffed in sp. grav. bottle, not in bleeding-cup	1038	—	3.4
9	25	Acute dysentery. V. S. to 2lb.; first portion not buffed, last portion, which flowed most freely, much buffed	1058	—	3.6
			1057	1024	4.0
10	30	Acute dysentery. V. S. to 26oz.; blood not buffed in the bleeding-cup, but distinctly so in sp. gr. bottle	1056	—	4.0
11	38	Intermittent fever, hot stage, with distressing retching and pain at scrobiculus cordis. V. S. to 1lb.; blood slightly buffed in bleed-			

TABLE continued.

No.	Age.	Disease.	Blood.	Serum.	Fibrin per 1000 parts.
		ing-cup, and distinctly in sp. grav. bottle; an appearance of air in the blood; it oc-curred in that which flowed last; a few small bubbles appeared to be disengaged, as it flowed into the cup, which was then close to the vein	1052	1025	3.9
12	26	Pain of side; little or no pyrexia. V. S. to 1lb.; blood slightly buffed . . .	1051	—	4.1
13	49	Rheumatic pains of three weeks' duration; blood slightly buffed	1047	—	3.6
14	25	Dislocation of hip-joint, from jumping out of window when drunk. V. S. to 4 lbs.; blood not buffed; serum subjected to distillation, yielded no perceptible traces of alcohol .	1054	—	—
15	40	Catarrhal symptoms, with stupor after exces-sive drinking for five days.[1] V. S. to 2oz.; blood slightly buffed; urine collected during the night, was of sp. grav. 1007; it was subject to distillation; no traces of spirit could be detected in what passed over; it was of the specific gravity of dis-tilled water.			.
16	35	In perfect health, (for comparison) . .	1052	—	4.5

The observations in this table were chiefly made in the mili-tary hospital in Corfu, and principally in the autumn. With one or two exceptions, the patients from whom the blood was taken, were soldiers of our regiments serving in the Ionian Islands. As regards salubrity, these islands may be considered as intermediate in degree between England and Ceylon; the average mortality in them amongst the troops being about 2.3 per cent. of the strength. Both the fevers and dysentery are less severe and fatal than the same diseases within the tropics, and especially the latter.

The experiments were principally instituted with a view to en-deavour to determine two doubtful points:—1st, whether there is any constant relation, between the specific gravity of the blood and its capability of yielding a buffy coat; and, 2dly, whether the pro-portion of fibrin is augmented in blood possessing this quality.

Relative to the first point, by one inquirer, it has been maintained that "blood which gives the fibrinous coat in a great degree, has a lower specific gravity than healthy blood;"[2]—at the same time re-marking, "that moderately sizy blood drawn from a person before

[1] According to the man's statement, he drank about two bottles of spirits daily, besides beer and porter. This case occurred in England, in an old soldier after just landing from India.

[2] Sir Charles Scudamore, Essay on the Blood, p. 35.

he becomes weakened, has not commonly a very low specific gravity."

The results of the trials contained not only in this table, but also in the two preceding tables, are not in accordance with the first conclusion. The only fair inference, it appears to me, that can be drawn from them, is, that there is no necessary, that is to say, no constant connection between the specific gravity of the blood and the presence or absence of the buffy coat. Confining the attention to the last table, it appears that in five instances in which the buffy coat was slight, the specific gravities were 1047, 1051, 1055, and 1054; in other five instances, in which the buffy coat was moderately thick, the specific gravities were 1041, 1033, 1052, and 1056; in one instance in which it was thick, the specific gravity was 1057; and in one, in which it was absent, (the blood of a person in perfect health) it was 1052.

It may be laid down, I believe, as a general rule, that the blood of persons labouring under acute diseases, differs in specific gravity very little from healthy blood; and is of comparatively high specific gravity, whether buffed or not—or at least not below the mean: whilst on the contrary, the blood of persons labouring under chronic diseases, attended with debility and wasting, is comparatively dilute, and of low specific gravity; and this, though most frequently associated with the property of having a buffy coat, is not necessarily so.

Moreover, in connection with the present subject, I would further remark, that I do not believe, that the time required for coagulation is in any wise constantly connected with specific gravity, as the same author asserts; his words are, "blood possessing the highest specific gravity coagulates the most quickly," and "that which gives the fibrinous coat in a great degree is slowest in coagulating."[1] The last portion of blood which flows from the vessels of a slaughtered animal, is of less specific gravity than the first; yet it coagulates more rapidly. The blood of the patient No. 8, in the last table, which was of the low specific gravity 1038, coagulated rapidly: on the contrary, I have known blood which differed very little from the healthy blood, and which did not exhibit a buffy coat, remain liquid eight minutes after having been drawn, and not till the tenth minute was its incipient coagulation distinct.

When the serum and coagulable lymph are not viscid, and in consequence not capable of supporting the red particles, these particles fall down rapidly; if the fluidity of the blood is great, two minutes are sufficient for their subsidence an eighth or even a quarter of an inch, as I have many times witnessed, and instances of which I shall hereafter bring forward. No doubt, very buffy blood is very slow in coagulating, for the red particles cannot sink far in a very short time; and it is well known that healthy blood, which is of comparatively high specific gravity, coagulates rapidly.

[1] Op. cit. p. 35.

It is my wish to be understood to maintain merely, that the quali-
ties in question—high specific gravity and rapidity of coagulation;
low specific gravity and slowness of coagulation—are not neces-
sarily connected, but rather accidentally, and consequently, that
neither can be considered as a general fact, and be announced as a
rule or principle.

Relative to the second point—whether the proportion of fibrin is
augmented in sizy blood, as is the opinion of the same author,[1] the
results of the trials, detailed in the last table, favour rather the con-
clusion, that there is no constant relation between the appearance
of the buffy coat of the blood and the proportion of fibrin which the
blood contains.

Sir Charles Scudamore, in the experiments from which he draws
the conclusion, that sizy blood contains a much larger proportion
of fibrin than healthy blood, estimated the proportion of fibrin not
for the blood as a whole, but for the crassamentum. Now, as sizy
blood commonly affords a contracted crassamentum in which the
fibrin is comparatively condensed, and in which the residual pro-
portion of serum is comparatively small—it follows that a definite
quantity of sizy crassamentum *must* contain more fibrin than the
same quantity of healthy crassamentum, and yet the healthy blood
may contain more fibrin than the sizy blood.

In the fifth example in the last table, the serum was decanted
from the crassamentum in the two successive instances of V. S.
and the red particles were expressed from it: the following were
the results—estimated per cent.[2]

Decanted Serum.			Expressed colouring matter and Serum.	Dried Fi- brin.
1st V. S.—1st Portion .	.	39.65	53.68	.478
2d Portion .	.	44.6	42.8	.37
2d V. S.—2d Portion .	.	30.3	30.8	.41

Now, doubtless, although the last portion of blood did not con-
tain more fibrin than the first, yet, had the estimate been made not

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[1] Op. cit. p. 119, et passim.

[2] The colouring matter mixed with the serum of the clot was obtained by
breaking up the clot in a moist linen bag, and subjecting it to a moderate
pressure by means of the fingers, till little but fibrin remained. The fibrin
was then well washed in the same bag until all the colouring matter was
removed, when it was collected, and, previous to being weighed, dried on a
platina capsule thoroughly at a temperature of 212°. And the same method
in relation to the fibrin was used in every instance in which the proportion
of it obtained is given in the table.

for the entire blood, but for the crassamentum, the proportion of
fibrin in this last would have appeared to have been very much
greater than in the first, in accordance with the preceding remark.

Regarding the results generally, the same remarks are applicable
to them, as to those contained in the second table ; and, independent
of the particular application, they are brought forward with the
same view. The only ones which appear to me to require any
special notice are those connected with the patients Nos. 14 and
15, from whom blood was taken after excessive drinking of spirits.

In these two instances I have mentioned, I could not detect alco-
hol ; in one case in the serum of the blood, and in the other in the
urine. It is an interesting and important question, what becomes
of the spirit taken into the stomach. Should other experiments
confirm the above, the unavoidable inference seems to be, that it is
decomposed.[1]

8.—*Observations on the formation of the buffy coat of the blood.*

In the preceding pages, I have endeavoured to show, first, that
there is no necessary connection between the buff on the blood, and
the specific gravity of that fluid; and, secondly, that there is no
necessary connection between the quantity of fibrin in the blood,
and its tendency to exhibit the buffy coat.

The circumstances, or qualities, on which the formation of the
buffy coat depends are, I believe, chiefly those so sagaciously and
ingeniously pointed out by Hewson, and principally confined to the
coagulable lymph, little dependent on the serum and red particles
which may be considered as passive, in a manner, rather than
active.[2] The circumstances or qualities alluded to, belonging to
the lymph, are its coagulating more slowly than in healthy blood,
and its possessing greater tenuity or liquidity, not only than lymph

[1] Recently, I am informed, in instances of death from alcohol, either taken
by men in excessive quantity, or introduced into the stomach of animals, it
has been detected in the blood, but in a larger proportion in the brain. The
author of these interesting results is Dr. Percy, who, greatly to his credit,
obtained them whilst a student at Edinburgh, during (if I have been rightly
informed) the past year, 1838.

[2] As the serum of the blood and its red particles appear to be subject to
some variations, they may occasionally in a minor degree influence the pro-
duction or prevention of a buffy coat. The serum is naturally in a certain
degree viscid; an increase of this quality, *cæteris paribus*, would retard or
prevent the appearance of the buffy coat, and *vice versa*. The red particles
are naturally of comparatively high specific gravity, owing to which they
rapidly subside, in a fluid of no or of little viscidity; any diminution of this
quality, whether from expansion of the corpuscles, as from a kind of dropsi-
cal state, or from greater buoyancy from adhering particles of air, also,
cæteris paribus, would have a similar retarding or preventing effect on the
formation of a buffy coat ; but, let me remark, these are hypothetical consi-
derations.

in its healthy state, but even than serum; in consequence of which the red particles more readily subside, and the lymph coagulating, free from the presence of colouring matter, appears colourless, constituting the buffy coat.

Of late years, this view of Hewson's relative to the formation of the buffy coat, has commonly been only partially adopted, and the circumstance, viz. the slowness of coagulation, which he held to be least important, has been most insisted on, and has been considered most essential.   Even Professor Müller is not an exception in this respect; in his Elements of Physiology,[1] he enumerates as the principal causes of the subsidence of the red particles and the formation of the buffy coat in inflammatory blood, not an increased tenuity of the coagulable lymph, but the slow coagulation of the blood or lymph, and an increased quantity of fibrin.

In a paper on this subject which was published in the Philosophical Transactions for 1822, I endeavoured to call attention to Hewson's original views, and to show, that in the production of the phenomenon in question, diminished viscidity is more concerned than diminished coagulability, using the expression in relation to time.   And in support of this theory, I adduced some instances, in which the buffy coat appeared on blood which was not marked for slowness of coagulation.   I stated, that in certain cases "in which the inflammatory diathesis is best marked, the separation of the red particles from the blood drawn is most rapid, often occurring in one or two minutes; and, that in some diseases, particularly erysipelas, the blood taken from a vein coagulates as rapidly as healthy blood, and yet exhibits the buffy coat.   In instances of this kind (it was remarked) when I have watched the coagulation of the blood, the red particles have subsided in the short space of two minutes, leaving a supernatant stratum of coagulable lymph, transparent and liquid.   The buffy coat, in these instances, did not appear on the blood collected in the common bleeding cups—only when small vessels, as wine-glasses or small gallipots, were used, and quickly filled and set aside to rest."   And, I added, as a general inference from the observations also in conformity with Mr. Hewson's views, "May it not be inferred generally, that the buffy coat is principally owing, not to the slow coagulation of the blood on which it appears, but to its increased tenuity, or, in other words, to the diminished viscidity of coagulable lymph, the effect of morbid vascular action, connected with the inflammatory diathesis?"

Mr. Hay, in his "Observations on the Blood," had objected to Mr. Hewson, that the coagulable lymph is not itself attenuated in inflammatory diseases, and that when it appears to be so, it is from dilution with serum.   Were this opinion correct, I remarked in the same paper, such blood should be of a specific gravity unusually low, which it is not, as I had satisfied myself by numerous experi-

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ments, made both at home and in Ceylon, and of which experi-
mental proof has already been afforded.

9.—*Historical Notices respecting the Blood, particularly its coagulation.*

A carefully drawn-up history of the speculations which have
been formed, and of the observations which have been made by
ingenious men, relative to the coagulation of the blood, and the
formation of the buffy coat, would be curious and interesting in
many respects ; and it may be noticed as a desideratum in medical
literature.

Aristotle appears to have attributed the effect of coagulation to
the presence of a fibrinous matter in the blood, either in the state
of minute solid fibres, or liquid with the power of forming fibres.
From an incidental expression,[1] it is not improbable that the latter
was his opinion ; and that he considered the matter liquid whilst
the blood was in motion in the living animal, like the matter of
curd in uncoagulated milk ; and, solid only and fibrous, after extrac-
tion, the coagulation being the effect of the change.[2]

[1] Meteorol. lib. iv. c. 7. D.

[2] Harvey thus gives the opinions entertained by Aristotle respecting the
blood : " Aristoteles (a) quoque ut sanguinem alendi gratia institutum puta-
vit, ita eundem etiam veluti è partibus compositum censuit. Nempe ex
crassiore et atra, quæ in fundum pelvis inter concrescendum subsidit, eaque
pars illi deterior habetur (b) : sanguis enim, inquit, si integer est, rubet et
dulcis saporis est ; sed, si vel natura vel morbo sit vitiatus, atrior cernitur.
Ex parte etiam fibrosa sive fibris constare voluit : iisque demptis, ait (c),
sanguis neque concresit neque spissatur. In sanguine præterea saniem agno-
vit : Sanies, inquit, sanguis incoctus est ; aut, quia nondum percoctus, aut
quod in seri modum dilutus fuerit. Atque hunc frigidiorem esse ait : fibras
autem partem sanguinis terrenam esse statuit." (d)

There are other particulars mentioned by Aristotle respecting the blood,
not uninteresting in the history of this fluid, and clearly showing what
great attention he had paid to it :—the following are some of the more re-
markable.

" Sanguis in primis necessarius communisque sanguineis omnibus ani-
malibus est, nec adventitius suppetit, sed vernaculus, atque intimus in
omnibus integris, atque imputidus habetur. Venas (he included the aorta
and its branches) hic vasa sibi, et conceptacula habet ; nec in ullo alio nisi
in corde, præter venas continetur. Tactum nullo in genere sentit ; sicut
excrementa quoque in alvo contenta, sensu carere certum est. Quinetiam
cerebrum et medulla tactum non sentiunt. Copia sanguinis in primis ani-
malibus, iis, quæ animal et intra se formant, et in lucem edunt : mox iis
sanguineis, quæ pariunt ova. Palpitat intra venas sanguis omnium anima-
lium pulsuque simul undique movetur, solusque omnium humorum sparsus
per totum corpus animalium est. Et semfer quandiu vita servatur, sanguis
unus animatur et fervet. Oritur primum in corde, antè, quàm totus corpus

(a) De Part. Anim. lib. ii. c. 3.
(b) De Hist. Animal, lib. iii. c. 19.
(c) De Hist. Animal, lib. iii. c. 19.
(d) De General. Animal, 402. (Op. Om. a Col. Med. Lond. edit.)

Harvey appears to have adopted an opinion on the subject very similar to that of Aristotle;[1] as did Sydenham also some years later; but with this difference and refinement,—an anticipation of a recent doctrine already abandoned, that the buffy coat is formed of the substance of the colouring matter, deprived of its pellicle.[2]

Amongst the distinguished physiologists, the immediate successors of Harvey, Lower holds a high place; and he appears to have been one of the earliest, who had a more precise and a more accurate knowledge of the blood, than was then current, or had descended from antiquity.   He seems to have considered the serum of the blood in the light of a vehicle: "Propter seri defectum,

formatur. Si multum sanguinis effluat, anima deficit : si nimium, vita interit. Si sanguis immodicè humescit, morbus infestat : sic enim in speciem saniei diluitur, et adeo serescit, ut jam nonnulli sudore cruento exundarint. Idem etiam interdum causæ est, ut qui effluxerit, aut omnino concrescere nequeat, aut incomptè, particulatimque spissatur. Dormientibus porio sanguinis copia partes exteriores deserit, subitque interiores, ita ut adacto cultello non æque effluere possit. Minus sanguinis per summa corporis fæminarum est : plus enim parte interiore continetur, et quæ menstrua appellamus, plurima fieri in mulieribus solent : quod genus sanguinis vitiatum, ægrotansque, fluit immodicè, atque profluvium ob eam rem dictum est. Cæteris morbis sanguinis minus mulieres infestantur, quam viri. Paucis item mulieribus fiunt mariscæ, aut varices, aut è naribus profusio sanguinis. Et si quid eorum acciderit, detrimentum in menstrua decumbit. Ætatum etiam ratione sanguis discrimen, tum in modo, tum in genere recipit. Etenim in ætate admodum juvenili saniem trahit, et largior est : in senectute autem crassus, niger et paucus. Medium tenet in media, firmâque ætate. Concrescit etiam facilè senum vel in corpore sanguis, qui per summa est : quod idem nunquam juvenibus evenit. De Hist. Animal, lib. iii. c. xix.

[1] In the following passage, Harvey appears to give his opinion on the nature of the blood, founded on that of Aristotle ; but modified, and approaching a little nearer to the present state of our knowledge on the subject. " Hic enim (sanguis) ut est naturale quid, heterogeneum sive dissimilare exsistens, in succis aut partibus illis componitur. Prout autem vivit, parsque animalis præciqua est, ex succis iis simul mistis constans ; est pars similaris animata ex anima et corpore composita. Evanescente autem, ob exstinctum calorem nativum, illa sanguinis anima, substantia hujus nativa illico corrumpitur et dissolvitur in ea, ex quibus olim constituebatur : primo scilicet in cruorem, postea in partes rubras et albas ; partesque rubræ superiores sunt floridiores, inferius autem sitæ nigricant. Partes poiro aliæ fibrosæ sunt et densiores (reliquarum vesiculum) : aliæ ichorosæ et serosæ, quibus coagulatus thrombus innatare solet. Atque in hoc serum sanguis tandem fere totus degenerat. Partes autem istæ non insunt vivo sanguine, sed a morte solum corrupto et jam dissoluto."—Op. Cit. p. 404.

[2] Sydenham thus expresses himself on the subject of the buffy coat : " Sanguis in pleuritide saltum ille, qui post primam vicem extrahitur ubi refrixerit, sevi liquati præ se fert speciem ad crassitiem satis conspicuam, ac superficiem habet veri *puri* æmulam, et tamen ab eo longe diversam utpote quæ fibris, instar reliqui sanguinis, arctè contexitur, nec non ad modum puris diffluit, quin à reliquo divulsa discolor illa pars, formam cuticulæ tenacis et fibris refertæ exhibit ; et, fortasse nihil aliud est, quam fibræ sanguineæ, quæ rubicundo ac naturali suo integumento per præcipitationem exutæ, ambientis aëris frigore in membranam hujusmodi subalbidam concrevere."—Op. Univers. Lug. Bat. 1741, p. 264.

sanguis, vehiculo suo spoliatus, circulatione ineptus redditur."[1] He was not only acquainted with the coagulability by heat of serum; but also of the liquor pericardii, and of lymph,—" lympha è glandulis secreta."[2]   He appears to have been well acquainted with the existence in the blood, of a glutinous fluid, its nutritive and its coagulable part,—to which the coagulation of the blood is owing, and many morbid phenomena are referable; thus, at page 130 of his work on the heart, he speaks of it, as "succus nutritius in sanguine admodum glutinosus;" and which "sensim concrescit, et pro cordis et vasorum continentium figurâ, variè configuratur, unde pro cordis polypo, verme et serpente interdum habitus est."

Borelli about the same time, or shortly after, entertained the same view of the blood, and announced it with even more precision, and illustrated it by experiment.[3]   He had come to that conclusion also, which I have inferred Aristotle had formed,—that the glutinous part of the blood, in the living animal, is liquid ; he was led to it by considerations connected with microscopical observations : " ideo fatendum est, gluten album sanguineum lubricam et fluidam consistentiam retinere dum in animale vivente movetur."

These just views may have been adopted by a small number of individuals, but they seem to have made no general impression at the time, or to have been objected to, or rejected, as if erroneous.[4] For a considerable period after Harvey's great discovery,—for nearly a century, whilst many branches of physical science were most successfully cultivated, comparatively little advance was made in the knowledge of the true nature of the blood.   The attention of the philosophical chemists was then chiefly directed to the effects of heat on it, and to the products obtainable by distillation ;[5]   whilst

[1] Tractus de Corde, p. 131.—London, 1669.

[2] Op. cit. p. 6.

[3] " Primaria pars rubicunda constat ex glutinossisima quadam substantia lubrica, et ex succo quadam purpureo, qui simul implicati sponte conscrescunt in grumos ; et post concretionem crebris aquæ ablutionibus apparet pars glutinosa, constipata in fibras candidas, vel in membranas recticulares similes tendinosis, ut in sanguine columbino patet ; pars verò rubra grumefacta, et abstersa ab aqua, abit in pulverem rubicundissimam, in fundo aquæ subsistentem."—Borelli de Motu Animalium, p. 192, (1685.)

[4] Haller, in his Comment. on Boerhaave, vol. ii. p. 310, published in 1742, referring to the opinion that the clot contains fibrous matter,—the opinion, he says, of Malpighi, Michelotti, Guglielmini, Hacquet, and of Adams ; observes, "Nerum, ne errori nascatur locus, fibrosa ea substantia non alia est ab ipsis globulis dictis (i. e. of Leeuwenhoeck) et evanescit solo calore."

[5] Macquer, one of most philosophical chemists of his time, in his Elements of Chemistry, published towards the middle of the last century, speaks of the analysis of the blood, as " a sort of general, though imperfect, analysis of the animal."   Speaking of the blood out of the body, he notices only " its yellowish serum or lymph," and the red curdled part which swims in the midst thereof.   He adds, " these two substances, when analysed, yield nearly the same principles ; and, in that respect, seem to differ but little from each other."—Elements of the Theory and Practice of Chemistry, Eng. Trans. 1764.

that of the natural philosopher and physician was directed, prin-
cipally, almost exclusively, to its red particles, to which they at-
tached vast importance, making their microscopial observations
the foundation of various speculations and wide-reaching hypo-
theses.[1]

At what time exactly the more precise ideas on the subject began
to prevail,—those which are inculcated in the writings of Hewson
and the Hunters, and some of their able contemporaries, it is not
easy to determine.   The term "coagulable lymph" was then in
common use, according to Hewson's definition of it; thus, Sir John
Pringle, in the description of a fatal case, in which the heart was
found to adhere to the pericardium, says, it was "in such a manner
as parts adhere from recent inflammation, that is, by an exudation
of coagulable lymph."[2]   The probability, however, it appears to
me, is, judging from the writings of Jurin,[3] Haller,[4] and Marherr,[5]

[1] Boerhaave, opposing the hypothesis of Galen, that the blood is composed
of four peculiar humours, remarks, "it appears clearly from the experiments
of Leeuwenhoeck, that the red clot (the first of Galen's Elements) is com-
posed merely of red particles united so as to form one mass; that the yellow
bile (his second Element) is merely serum, and that its more dilute and
pellucid parts (his third Element, in which the yellow bile was supposed to
be dissolved) is to be considered as phlegm ; and that the black parts of the
crassamentum, its inferior portion (his black bile, and fourth Element) does
not differ from the red, excepting it not being exposed to the air."—Prælect.
Acad. 1. 328.   The elaborate medical doctrines which this distinguished
man built up, on the incorrect microscopical observations of Leeuwenhoeck,
afford the most remarkable instance of what has been stated in the text; and
should ever be a warning in refined and obscure inquiries, in which the
microscope is brought into use.

[2] Hewson, in his Treatise on the Lymphatic System, published in 1764,
gives the case at page 114, transcribed from Sir John Pringle's notes.

[3] Dr. Jurin, in his able paper, in the Philosophical Transactions for 1719,
on the specific gravity of the blood and its different parts, distinguishes only
the red particles and the serum.   He was of opinion that the crassamen-
tum is composed "of the globular particles united together by their attractive
power."

[4] Haller, in the second volume of his Elementa Physiologiæ, published in
1760, at page 42, remarks,—"Solent Medici Galli nuperiores (Petit Mem.
de l'Acad. des Scien. 1732, p. 393; Senac, t. ii. p. 92,) monere, duriora
coagula esse, quæ ex sero nascuntur, et serum vi densabili cruorem superare,
cum in frigida coeat, dum sanguinis pars rubra conservat." "Verum certum
est," he continues, "etiam ex rubro cruore, dura nasci coagula, et quidni
nascantur, cum in ea sanguinis parte, et terræ et ferri, plurima sit portio."
So vague is this great physiologist on the subject.

[5] Marherr, in his admirable "Prælectiones Medicæ," published in 1772,
probably expressed the doctrines then current, and considered most orthodox,
in the continental schools, at least in the North of Europe; he attributes the
buffy coat of the blood to a change in the serum.   "Augetur vero visciditas
illa seri sæpe tantum ut etiam more cruoris protinus coagulatur et ipsum
dum sanguis de vena missus coit. Atque tum cruor concretus pellicula alba
densa, tenace tegitur, quæ ex sero viscidore nascitur, et crustæ pleuriticæ
nomine insignitur, quoniam," &c. tom. ii. p. 254. The crassamentum, he
considered, composed of red particles adhering together; "thus," he says,
"coagulum hoc rubrum minui sensim mole et mora temporis in vase sero-

that the views on the subject of the blood entertained by our great
English physiologists last mentioned, (passing by the earlier and
similar opinions, which never took root, and seem to have been
lost,) belonged to their time, and chiefly to our country ; and that
to them, and perhaps to one or two French inquirers, belong the
merit of reviving, or rather of originating, them *de novo*, and of
proving them true.

One or two remarks of Hewson's seem to point to this, and in a
manner claim the honour of discovery; thus, in a foot-note, at page
51, of his " Experimental Enquiry into the Properties of the Blood,"
after having detailed some of the most remarkable of his results, he
observes, " that most of the facts which occur in the preceding
pages have been mentioned in my Anatomical Lectures, ever since
the year 1767 ; and some of them were mentioned publicly even
before that time."   And he adds, " This I thought it necessary to
observe, because many of them have since appeared in other publi-
cations." Thus, in another place, also in a foot-note, after describing
the crassamentum, which, he says, " it is well known consists of
two parts, of which one gives it solidity, and is by some called the
fibrous part of the blood, or the *gluten*, but by others with more
propriety termed the *coagulable lymph*," remarks, " It may be
proper to mention here, that till of late, the coagulable lymph has
been confounded with the serum of the blood, which contains a
substance that is likewise coagulable.   But in these sheets, by the
lymph is always meant that part of the blood which jellies, or be-
comes solid spontaneously, when blood is received into a basin,
which the coagulable matter that is dissolved in the serum does
not, but agrees more with the white of an egg, in remaining fluid,
when exposed to heat, or when mixed with ardent spirits, or some
other chemical substances."[1]

The views of Hewson, on the subject of coagulable lymph, on
the coagulation of the blood, the formation of the buffy coat, the
contents of the lymphatics, on the analogy between the coagulable
lymph which they contain, and the coagulable lymph of the blood,
are so clear and precise, and deduced from such admirable experi-
ments, that it appears not a little difficult to comprehend, how they
could be either overlooked or not perfectly understood or appre-
ciated. And yet, that they have been, is the unavoidable impression
received in reading some of the works of the able physiologists of
the present day.   Justice has not yet been done to the high merits
of Hewson, either as a most acute and accurate observer, or as a
very original inquirer.   Dr. Baly, in his translation of Professor
Müller's work on Physiology, in a note to that part of the text in
which it is said, that Berzelius suggested the idea that the clot is
composed of fibrin enclosing the red particles, supports the just

sum liquidum augeri, quod antea in interstitiis coaguli interclusum, per
moram et quietem magis sese attrahentibus moleculis rubris, ex hisce inter-
stiis exprimitur, dum illæ ad se proprius accedunt."

[1] Op. cit. p. 6.

claims of his countrymen, and in part those of Hewson, but not, I think, to the full extent of his merits. Hewson's ideas on the nature of coagulable lymph, founded on the results of his experiments and observations, appear to me as clear as possible—even as clear as those of Professor Müller himself, and that he would have made the same ingenious experiment in confirmation of them (I allude to the separation of the fibrin of the blood of the frog from its red particles, by filtration in its liquid state) had he been aware that these particles, on account of their large size, could be detained on a filter.

As Dr. William Hunter was senior to Hewson, and as the latter was associated with him, it may be a question, whether the merit of the discovery of coagulable lymph is not due to him.

In his controversy with the late Dr. Monro, published in 1777, under the title of " Medical Commentaries," he gives a quotation from his lectures, delivered in 1759–60, from which it appears, that then he had a clear apprehension of the part performed in the blood by coagulable lymph. I shall transcribe the passage, as it is on many accounts interesting. Accounting for the red blood not transuding through the vessels, he refers it principally to the glutinous quality of the blood, when " equally mixed up with its coagulating part." " That part," he adds, " coagulates as certainly as the blood stagnates, even in living bodies ; and when the universal stagnation happens in death, this part of the blood collects itself into irregular *polypi* and coagulations all over the body, and the rest of the blood is no longer the thick viscid fluid it was before, but rather a bloody *serum* that will ooze through all the vessels and membranes."

Now as Hewson, at this time, was only about 20 years of age, (according to Mrs. Hewson, he was born in November, 1739, and was in partnership with Dr. Hunter in 1768,[1]) Dr. Hunter's knowledge of the nature of coagulable lymph could hardly have been derived from Hewson, and it must be considered either as the result of his own observations, or of observations common to him and his contemporaries, and not claimed by any one in particular, which, on the whole, perhaps, is most probable. Still, however, to Hewson, I believe, belongs the merit of elaborating the doctrine, and of making precise what was before vague.

In mentioning the revival of correct ideas on the subject of the coagulation of the blood, I have alluded to one or two French inquirers, who perhaps may be considered entitled to share in the merit I have attributed to our English physiologists, if not to have anterior and superior claims, particularly those very able and distinguished men, Petit and Senac. They certainly preceded Hewson, in adopting Borelli's view, or the analogous view alluded to, of the nature of coagulation of the blood, as solely depending on a white coagulable part, which they called lymph ; but I am not aware that

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[1] Biograph. Med. 1, 428.

either of them brought forward in support of it any facts, or experi-
mental results more decisive than those of the Italian physiologist;
nor do their statements appear to have had any influence on the
doctrines of the day, judging from the opinion of Haller, already
alluded to.   Below, I shall give some extracts from their writings,
expressing their ideas on the subject.[1]

[1] Petit, in his second " Mémoire sur la manière d'arrester les Hemorragies,"
published in Mem. de l'Acad. des Sciences for 1732, makes the following
remarks:

" Attendu que la lymphe est plus légére, la moitié superieure de ce caillot
est blanche, et l'inférieure est d'un rouge-brun."

" Si l'on examine le bassin dans lequel on vient de saigner du pied, on
trouvera toutes les parties du sang noyées dans l'eau chaude, et si l'on veut
voir à l'instant quelle est la partie du sang susceptible de coagulation, on n'a
qu'à jetter un pot d'eau froide dans le bassin, et sur le champ, on verra la
partie blanche se séparer de la partie rouge, et s'élever sur la surface de
l'eau où elle forme des caillots trés-durs, pendant que la partie rouge
demure universellement et exactement mêlée avec l'eau, et sans former
ancuns caillots.

De ces expériences connuës de tout le monde, on peut conclure que la
partie blanche est non-seulement plus disposée à la coagulation que la partie
rouge, mais qu'elle est la seule qui se coagule, et que la partie rouge ne feroit
point partie du caillot, sans la partie blanche qui la retient.   Les différents
degrés de consistence qu'on trouve dans les caillots en sont une seconde
preuve.   En effet, le caillot de toute la masse exactement mêlée a quelque
consistence, mais lorsque la partie rouge et la partie blanche se sont coagu-
lées, pour ainsi dire, séparément ; le caillot blanc est très dur, parce qu'il ne
contient point de partie globuleuse, et le rouge est d'autant plus mol qu'il
contient peu de lymphe ; du manière que quand la partie globuleuse et la
sérosité restent fluides, et que la lymphe se coagule, le caillot est encore plus
dur et plus blanc : ainsi les différents degrés de blancheur et de solidité des
caillots dépendent du plus ou du moins de parties gobuleuses que la lymphe
retient, en se coagulant."—p. 393.

Senac, in the first edition of his Treatise on the Heart, published in 1749,
(vol. ii. p. 660,) speaks of lymph, incidentally, in connection with the colour-
ing particles, when refuting some errors of Leeuwenhoeck relative to the
latter.   He mentions it as " cette matière blanche, qui se coagule d'elle-
même et qui forme la cóene du sang des pleurétiques."   He continues,
" J'ai d'abord voulou l'examiner quand la sang s'en sépare et se refroidit ;
mais on ne peut pas la saisir exactement ou elle est encore fluide.   J'ai donc
pres cette matière figée et fort blanche ; mais je n'y ai vû aucune trace des
globules."   He does not claim to himself the discovery of the coagulability
of lymph ; more than he does the globularity of pus, the globules of which,
as seen under the microscope, he correctly describes as larger than the blood
particles.

The next passage I shall transcribe is from the second edition, which
appeared in 1783, after the death of the author ; he thus opens the 13th
chapter of the 5th book, on the causes which keep the blood liquid, " Cet
assemblage de matières inégalement pesantes, n'est qu'un fluide artificiel, ou
pour mieux dire ce n'est pas un corps fluide ; il n'est qu'une matière fondue
par diverses causes ; sa liquidité ne subsiste qu'autant que ces causes agis-
sent ; abandonné à luimême il se coagule dans ses propres vaisseaux ; si on lie
une carotide en deux endroits, dans un animal vivant, le sang se condense
entre les deux ligatures ; si les issues du cœur ne sont pas libres, il peut se
former des concrétions dans les ventricules ; elles ne sont pas rares dans
l'aorte, lorsxu'elle se dilate.

Should the historical desideratum to which I have alluded, in introducing these brief notices, be supplied, written with the scientific accuracy and philosophical spirit suitable to the subject, it will prove, I apprehend, no less curious than instructive: the history of this our vital fluid, will be any thing but a triumph of human intellect; it will show how difficult is the attainment of physiological knowledge, (considering merely the coagulation of the blood,—as it were, a single quality;) how insecure is the tenure of this knowledge; how unstable and fleeting is opinion; and how prone the philosopher has been to adopt wild and fantastic notions; and to fall into almost absurd errors, when deviating from the rigid inductive method.

II.

MISCELLANEOUS OBSERVATIONS ON THE BLOOD.

Most of the following observations were made between the years 1824 and 1833, in the Mediterranean, chiefly in Corfu and Malta; and were published at intervals in the Edinburgh Medical and Surgical Journal. They are now given in many instances more in detail, on the persuasion, that to be useful, detail is necessary, even at the risk of tediousness.

1.—On the effect of violent agitation on the Blood.

Dr. Bostock, in the first volume of his Elementary System of Physiology, remarks, "It is well known that if blood, as it is discharged from the vessels, be briskly stirred about for some time, the process of coagulation is entirely prevented from taking place, either in consequence of a more complete union of its parts with each other, which prevents their future separation,—or from the

Toutes les parties du sang ne forment pas par elles-mêmes le fonds de ces concrétions ; c'est la lymphe seule qui se coagule, et qui en se coagulant, es le lieu des autres matières qui y sont mêlées ; elle lie, par example, avec elle les molécules rouges et leur donne de la consistance ; si elles étoient seules elles ne s'attacheroient point les unes aux autres ; dès quelles sont séparées du reste, elles flottent dans l'eau ; ou si elles se rapprochent, leur cohésion est légere ; elles retiennent même qulque portion de matière lymphatique qui les unit ; aussi leurs concretions sont elles tendres, et semblables à une gelée presque liquide."

Hewson's Experimental Inquiry into the Properties of the Blood, was published three years before the last edition of Senac's work, viz. in 1780, with the very characteristic motto, both as regards the inquiry itself and the author's own mind, " *Vere scire, est per causas scire,*" words of the author of the Novum Organum.

fibrin after it has been for some time discharged from the blood, losing this peculiar property by which its particles are attracted together."[1]

To ascertain if a brisk motion really prevents the coagulation of the blood, as is asserted by Dr. Bostock, I made the following experiment, preferring it to be the means mentioned by him, as we know that by moderately stirring the blood, its fibrin merely is separated, and not its property of coagulating destroyed. About two ounces of blood were received into a large vial and immediately shaken violently, and the agitation was continued without intermission for ten minutes, which was two minutes after the blood at rest coagulated. The result was, that the blood thus shaken acquired a scarlet hue from being mixed with, and from the action of air, and *appeared* to be liquid. But this was merely in appearance; for when poured on a filter, it separated into two parts,—one, the serum, containing, suspended in it, the red particles, which passed through the filter:—the other, the fibrin in a finely divided state, coloured by adhering red particles which remained on the filter. This effect of agitation may be imitated by breaking up the crassamentum of the blood with the fingers, when an apparent solid is rendered an apparent liquid. Nor is this at all surprising, considering the small quantity of solid matter contained in the crassamentum, to which it performs the part, as it were, of a cement, or of calcareous matter to sand, combined with which the sand constitutes a solid rock, and deprived of which it is as a yielding quicksand, almost as fluid as the red particles of the blood deprived of fibrin.

In confirmation of this explanation, I may add, that blood rendered apparently liquid, as above described, when examined by the microscope is found to contain, besides the blood-discs, which are little altered[2] floating in the serum, masses of fibrin, of irregular form and structure,—(neither distinctly globular nor fibrous) and particles also of irregular form, very much smaller than the blood-discs. On rest, after a few hours a separation takes place,—the red particles, in consequence of their greater specific gravity, subside towards the bottom, and the particles of fibrin, owing to entangled air, collect at the surface.

2.—*On the effect of moderate agitation on the blood.*

Sir Charles Scudamore, in his Essay on the Blood, endeavours to show that moderate agitation of the blood favours its coagula-

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[1] Lower supposed that agitation had this effect on blood, and in his time, (now at least 170 years ago) the opinion appears to have been current: thus, when on the subject of transfusion, and considering the best state of blood for the purpose, he gives the preference to that flowing from a living animal, because, he remarks,—the blood which has been collected in a vessel, (utcunque satis præcaveri posset, crebrâ agitatione, ne congeletur) is necessarily in some respects changed.—De Corde, p. 173.

[2] The discs after agitation are not quite so regular in form as before.

tion,[1] and, conceiving that his experiments prove the fact, he accounts for it on the idea, that agitation accelerates the escape of the car- bonic acid gas, which, as he believes, the blood contains, and on the escape of which, he is of opinion the phenomena of coagulation depend.

It may appear a most easy matter to determine whether moderate agitation does or does not accelerate the coagulation of the blood, the experiment is so simple, and so easily made ; and yet, I must confess I have experienced considerable difficulty in arriving at a conclusion from the contradictory nature, not of one or two, but of many results. The great difficulty in this inquiry, and in most in- quiries relative to the blood, is, the impossibility of knowing that any two portions of the same fluid are exactly similar. My opinion is, that no two portions are, and even that no two drops of this fluid are exactly similarly constituted, or possessed of exactly the same properties ; and that the portions of a continuous stream flowing from the arm, were it divided into a hundred, may all be different. The results of experiments first led me to adopt this notion, in which I was confirmed by further experiments, and by reflecting on the subject. Not to be tedious, I shall mention only one experi- ment. Six gallipots of the same size, and a platina crucible of nearly the same size, were filled in succession with venous blood as it flowed from the arm of a soldier labouring under a slight ailment, and on whom no well-marked effect connected with loss of blood was perceptible during the operation. They were placed close to each other, and carefully observed. The blood in each did not co- agulate, as might have been expected, in the order in which it was received, but irregularly. Thus the first received was the last but one to coagulate, and that which was received in the crucible, which was the fourth in succession, coagulated first. When one reflects on the subject, the inference appears to me to be obviously what I have suggested above, viz. that no two drops of blood are precisely similar in their composition and properties. It is evident on reflection that the fluid of a continuous stream of blood must be derived from different parts of the body. In one portion there may be an unusual or less than usual quantity derived from the vessels of the muscles, or brain, or liver, or spleen, &c. ; and we cannot but suppose, that the blood from the kidney, after urine has been se- creted from it, is different from the blood from the liver or pancreas, after supplying bile and the pancreatic fluid. The preceding exper- iments and this reasoning are confirmed by another fact which I have observed ; viz. that if the crassamentum, the instant it is formed, is removed, it will be succeeded by another coagulum ; and if this is removed, by another—and so on repeatedly till all the fibrin has coagulated,—clearly demonstrating, that in any given quantity of blood, there are particles of fibrin of various degrees of rapidity of coagulation. And is it not also confirmed by microscopical obser-

vation? If we examine, with a powerful microscope, a minute portion even of a drop of blood, as indeed is necessary; is there not a variety observable in the particles, either as regards the outlines of the discs, their thickness, or their other dimensions? The resemblance of one to the other is general and considerable; but not without, as it has appeared to me, slight differences.

To return now to the effect of moderate agitation on the coagulation of the blood,—it may be easily understood how seemingly contradictory results may be obtained, if the effect is not very well marked. It certainly is not well marked. In some instances it has appeared rather to retard; in others, neither to retard nor accelerate, and in others, and these the majority, to accelerate coagulation.

As the majority of the experiments which I have made indicate this, I infer that agitation does promote the coagulation of the blood out of the body. But admitting it to be a fact, I cannot adopt Sir Charles Scudamore's explanation of it, as it does not appear to me proved that any carbonic acid gas is disengaged from blood in the act of coagulation; or if it were, that its removal would have the effect imagined. I believe it is owing to a very different cause. I find that when a small portion of crassamentum is put into blood, the coagulation is distinctly accelerated, having, as it would appear, an effect similar to that of a crystal in a saturated saline solution ready to crystallise; and it has occurred to me that agitation may promote coagulation by producing a more intimate mixture of the particles, of those which are first coagulated, with those which are still liquid; I say "first coagulated," because it may be inferred from what has been already stated, that some particles of lymph are co-agulated almost immediately after the blood is drawn, and these may act on the mass like a portion of crassamentum introduced into it.

3.—On the effect of lightning on the blood.

It is commonly believed that the blood of those killed by lightning does not coagulate. The opinion has been adopted by Hunter,[1] but on what evidence is not stated. Now, as it appears that the blood of animals killed by the discharge of an electrical battery is not deprived of its power of coagulating, and as electricity, applied as a chemical agent, seems rather to promote than retard the coagulation of the albuminous contents of the blood, *à priori*, it does not seem probable that the popular belief is correct. The question, however, cannot be determined excepting by an appeal to facts. The following case, with this view, may be deserving of being recorded; I shall give it in detail, from my notes taken at the time, on account of the interesting nature of the subject.

During a violent thunder storm at Malta, on the 15th of Novem-

[1] Treatise on the Blood, &c. p. 34.

ber, 1832, a Maltese labourer, aged 65, previously in good health, was struck dead by lightning. The body was conveyed to the civil hospital, where I had an opportunity of examining it twenty-four hours after the event.

The hair on the left side of the chest was slightly singed. The cutaneous vessels generally were much distended with blood; the face and neck were purplish red; the inferior surfaces purplish. The fingers were contracted and *rigid*. The abdomen was very much distended, tense and tympanitic. There was froth at the mouth, mixed apparently with some of the discharged contents of the stomach.

On inspecting the abdomen, the stomach and intestines were found enormously distended with air. There was much air in the cellular structure from putrefaction; the cellular structure even in which the gall-bladder is embedded in the liver, was distended with air. There was a general staining of the different structures by the blood, and the blood-vessels, and the lining membrane of the heart and bronchia was dyed dark red. There was frothy blood in the veins, and in the right cavities of the heart with a little soft coagulum. The lungs were very red and distended with air, and its vessels were strongly stained.

The atmospheric temperature during the time that elapsed between the event and the examination, and the temperature of the room where the body was deposited, varied between 55° and 60°. Such advanced putrefaction as was witnessed, is uncommon in the time specified, in the hottest weather. At the moment,—reflecting on the degree of putrefaction, and the state of the blood, I had little doubt, that the latter was owing to the former, however that might be produced. This idea, I am still disposed to entertain, and even to extend to all similar cases. And the conclusion is the more probable, in relation to the like events in this country, in as much as thunder storms are of rare occurrence, and seldom severe, excepting in summer and autumn, and in the most sultry weather, when the tendency to rapid putrefaction is greatest.

It is mentioned in the account of the *post mortem* appearances, that the fingers were contracted and rigid. Hunter was of opinion, that the muscles of those struck by lightning are relaxed and entirely deprived of their tone. I am equally ignorant how he arrived at this conclusion, which I am disposed to think was peculiar to him, and, perhaps, founded on hypothetical views. Some of the older writers particularly insist on the rigidity of the muscles in those struck by lightning; and Mayow, in his *Tractatus Quartus de Motu Musculari*, endeavours to explain, by means of his very original speculations connected with the nitro-aërial principle, " *cur fulmine percussi obrigescunt,*" treating of the effect as if it was then generally acknowledged.[1]

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[1] Op. cit. cap. vi. ad finem.

4.— *Effect of hydrocyanic acid, and of violent and long-continued muscular exertion on the blood.*

It has been asserted that the blood of animals, hunted to death, and killed by the prussic or hydrocyanic acid, continues in the liquid state; and even the high authority of Hunter may be adduced in support of the opinion, that in animals run down in the chase, the blood is deprived of its power of coagulating. From all the information, however, which I have been able to collect, I am compelled to conclude that the opinion in both cases has been hastily taken up, and is not correct.

In relation to the hydrocyanic acid, in no instance have I witnessed the blood liquid or deprived of its power of coagulating, in animals poisoned by it,—and I have paid attention to the state of the blood in many animals thus killed,—the cat, the dog, the rabbit, —and the appearance which it has exhibited has been in no wise peculiar; its coagulation, as well as I could judge, was neither retarded nor accelerated. And this might be fully expected, *à priori*, considering that the hydrocyanic acid has no visible effect, even in large quantity, when mixed with the blood, out of the body.

Relative to animals hunted to death, the results of my inquiries has been very similar. From what I have been able to learn from sportsmen, and especially from some of my professional friends, familiar with field sports, coagulated blood is commonly to be found in the hearts of animals thus killed; and I am authorised by an observer, on whose accuracy I have perfect reliance, viz. Dr. Andrew Smith, to state that this is the result of his experience in Southern Africa, where the larger antelopes are frequently run down by dogs. Perhaps, in this instance, as well as in that of death from lightning, when a loose or liquid state of the blood has been observed, putrefaction has been more concerned in producing it, than the mode of death.

5.— *On the effect of change of temperature on the coagulation of the blood.*

It is agreed by all those who have made experiments on the subject, that cold, instead of being the cause of the coagulation of the blood, as was for a long time believed after Aristotle,[1] actually re-

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[1] Aristotle, speaking of the coagulation of the blood in connection with its cause, says, " Si fibras detraxeris, sanguis non cogetur. Ut enim si ex luto terrenam portionem semoveris, aqua non concrescit; ita sanguis, fibris detractis, incongelabilis manet; fibrarum enim terrena est. Quod si non eximantur, cogitur, ut terra humida, frigore. Cum enim calor exprimitur, humor circa trahitir, atque evaporatur: atque ita concrescit, non à calore siccescens, sed à frigore."—De Part. Animal. lib. ii. ch. 4.

Harvey, as appears from the passage already quoted, refers the coagulation of the blood to an escape of the native animal heat; and Sydenham to the cold of the atmosphere.

tards coagulation. My own results are in conformity with this, and with the results of the accurate Hewson, that the viscidity of the blood is, at the same time, increased. At the temperature of 32°, I have seen blood remain liquid more than an hour; a reduction of temperature, some degrees lower, was required to freeze it,—when it appeared as a homogeneous mass.

Hewson, from his experiments on the freezing of the blood, inferred, that its qualities were not thus altered; that when thawed, it returned to its liquid state, and that it was then subject to coagulate. He speaks very decidedly on this point: he says,—"the blood was always evidently fluid on being thawed,—and as evidently jellied when exposed to the air."[1]

Mr. Thackrah, from his experiments, infers the contrary, viz.— that by freezing, the blood permanently loses its power of coagulating.[2]

The few experiments which I have made on this subject, instituted long before I had seen Mr. Thackrah's book, perfectly agree in their results with those of Mr. Hewson. I shall describe them from notes taken at the time, in the month of January, 1828, at Corfu.

Received into a thin glass tube, the temperature of which had been reduced by ice and dilute sulphuric acid, some blood of a fowl, and immediately placed it in the freezing mixture. The blood was frozen in a few seconds. After three or four minutes it was taken out of the mixture; it presently became liquid. It was again replaced and refrozen. It was once more taken out, and it melted again; and it was again speedily replaced. At the end of four or five hours, left in ice thawing, and which was then thawed, I found it coagulated. Some of the same blood not cooled artificially, coagulated in less than a minute; and in less than three minutes, when received into a platina crucible, and suddenly cooled by ice, to 40°.

The subject of the next experiment was venous blood, from a soldier labouring under a slight ailment. About an ounce of this blood, received into a glass tube, was instantly put into a freezing mixture. After about ten minutes, a thermometer introduced into the blood fell to 26°. It remained at this temperature about an hour, and all the time liquid : the red particles had subsided, and the supernatant fluid was colourless. It was then put into a freezing mixture of greater strength, and placed under the receiver of an air-pump; the air was exhausted, and the pump was worked till the blood was apparently frozen. After it had been some minutes frozen, I took it out, and applying a thermometer, found the freezing mixture at 10°. On pressing the frozen white part, a little fluid rose from the cruor,—the temperature of which was found to be 32°.

[1] Experimental Inquiry, p. 28.
[2] An Inquiry into the Nature and Properties of the Blood, by C. T. Thackrah, 2nd edit. London, 1834, pp. 67, and 37.

The experiment was repeated till the whole was frozen. The lymph was kept in a frozen state about twenty minutes,—the cruor about three or four. The blood was now placed in a room, the temperature of which was about 55°. It gradually thawed, and, when thawed, was liquid,—perhaps a little thicker than when first drawn. From the time it was completely melted, it began to coagulate in about eight minutes,—and, in about ten minutes, a soft crassamentum was formed. On examination, the following day, the crassamentum was found soft and not much contracted; serum had separated from it distinctly.

These results sufficiently accord with the more numerous experiments of Hewson, and in consequence, I am more disposed to consider his inference just, than the contrary one of Mr. Thackrah. And if we examine the detail of the experiments of this ingenious inquirer, this conclusion, I think, will be strengthened. In one experiment, after having been thawed, he states that a small quantity of serum separated from the blood; in another, when the thawed blood was mixed with water and passed through a filter, some very small portions of coagulum were retained; and describing his results generally, he says, that the blood "on being afterwards thawed becomes a grumous mass, and does not separate into serum and crassamentum." Now, the grumous mass, and the other particulars mentioned, indicate at least a certain degree of coagulation. As in one experiment, he appears to have placed the blood to thaw in a warm room at 65°, and to have observed it eighteen hours after, it is not improbable, that the fibrin might have been in part reduced to the liquid state from putrefaction. Be this as it may, negative results must almost always be considered of inferior value and force than positive; and if exceptions are to be made, commonly there is more propriety in selecting, for this purpose, the former than the latter.

Admitting the accuracy of the above, it is probable, that blood may be frozen for any length of time, without losing its power of coagulating, and without losing its power of becoming liquid; a happy circumstance, in relation to the animal economy in these regions in which the cold of winter is severe, and exposed parts of the body are liable to be frost-bitten; and perhaps equally so, I may remark farther, is the circumstance of the fluidity of the blood being diminished by cold in relation to animals which hibernate during several months of the year in a state of torpor, in which life is barely maintained.

From the experiments which I have made on the effect of a high degree of temperature, as 120°; of a moderate degree, as 100°; and of degrees between this and the freezing point, I am disposed to infer that the first immediately renders the blood more liquid, and accelerates its coagulation; that the second rather retards than accelerates coagulation; and that at 80° or 90°, its coagulation is less rapid than at 120°, and more rapid than at 100°. It may appear a paradox, that heat above 100° should increase the liquidity

of the blood and accelerate its coagulation ; but is it more so than that cold should render it more thick and viscid, and yet retard or prevent coagulation ? I infer that the effect of heat is such as I have represented it, from observing blood heated to the degree mentioned, and which coagulated more rapidly than blood not so heated, covered with a buffy coat, when the cool blood, slower in coagulating, was free from it. The blood is a mysterious fluid ; nothing can with propriety be taken for granted respecting it. Its properties can be ascertained only by exact experiment and unbiassed observation. Every property which it possesses seems to be adapted to admirable ends ; and even these peculiarities in relation to the effect of the higher as well as the lower degrees of temperature in its coagulation may be considered as such adaptations. The effect of high temperature on coagulation may tend to limit the destructive effect of heat to the surface ; the effect of a temperature of 80° or 90°, which is about the temperature of the skin, may tend to suppress hemorrhage, the consequence of superficial wounds.

5.—Of the effect of different kinds of vessels on the coagulation of the blood.

It is generally supposed, that the kind of vessel in which the blood is received, its size, form, and quality, have a considerable influence in promoting or retarding its coagulation and the formation of a buffy coat. I have made many experiments to endeavour to ascertain the truth of this opinion ; but I have not yet been able to arrive at a satisfactory conclusion. I am rather disposed to think that wood and polished metals retard coagulation, and that glass and earthenware accelerate it. The form and size of the vessel will, of course, have some influence, were it merely in connection with temperature. The remarks which I have made relative to the difficulty of ascertaining the effect of moderate motion on the blood, are particularly applicable here ; and, *à fortiori,* as the effect of the quality of the vessels I have tried, is even more ambiguous than that of motion. In illustration I shall give the results of one trial on the same blood, with different vessels, extracted from my note-book.

On the 12th December, in the military hospital at Corfu, blood from the arm of a patient slightly indisposed, was received, first, into a wooden cup ; second, into a circular pewter one, divided equally into two compartments ; third, into a silver dish, placed on lint moistened with ether ; fourth, into a small cupping-glass. The coagulation of the blood first took place in the wooden dish, and not at the circumference, but in the *middle ;* next, in the semi-circular compartment of the pewter vessel, in that in which the quantity received was greatest, and in it also in the middle ; next in the silver dish at the surface ; and, last, in the other compartment of the pewter vessel, in which only a very small quantity, about four or five drops, had been spilt.

Perhaps it may be safely laid down as a general rule, that the
9—g   4 *day*

form and quality of vessel best adapted for preventing sudden cool-
ing, and therefore an increase of the viscidity of the blood or
coagulable lymph, favour most the rapid subsidence of the red par-
ticles, and consequently the formation of the buffy coat. This is in
accordance with Dr. Benjamin Babington's experience with thin
pear-shaped glass vessels, such as he used for the purpose of ascer-
taining the specific gravity of the blood ;[1] and also with mine, on
thin globular glass vessels. Very often I have found a buffy coat
in these globes, when none appeared on the blood collected in the
usual way.

7.— *On the influence of the blood-vessels or of vital action on the coagulation of the blood.*

This is allowed to be, and necessarily is, a point of inquiry of
great obscurity; both as regards the fluid itself and the causes re-
ferred to. I shall enter on it very partially.

Hewson, from his experiments and observations, inferred that
"the properties of the blood depend on the state of the blood-vessels;
or that they have a plastic power over it, so as to be able to change
its properties in a very short time;" that what increases or strength-
ens vascular action, or the heart's action, tends to retard coagula-
tion, and that which has a contrary agency, promotes and accele-
rates it.[2]

He arrived at this, which he calls his "most remarkable conclu-
sion," from witnessing the sudden change in the coagulating power
of the blood, during the same blood-letting, as exemplified in differ-
ent portions, received separately; confirmed by what he witnessed
in instances of animals blooded to death.

In these last mentioned instances, it has been shown, that the
specific gravity of the blood which flows from the slaughtered ani-
mal, when nearly exhausted, is less than of that which flows when
the vessels are first divided.

The circumstance necessarily gives rise to reflection. As it
seems to show that the composition of the blood is altered at least
as regards the relative proportions of its constituent parts, as ob-
tained in the process of draining the vital fluid, it is matter for con-
sideration, whether something similar may not take place in the
other instances referred to by Hewson ; and whether the changes
in the circulating mass which he attributed to the vessels, acting
on the blood, may not be owing to a vascular action more analogous
to that of secretion.

It is easy to conceive that, under different degrees of vital action,
the nature of the current of blood, either in the minute vessels,
arterial and venous, or in the capillary vessels connecting them, or
in both, may be modified ; that in one condition more red particles
may pass, in another a larger proportion of serum, in another of

[1] Medico-Chirurg. Trans. vol. xvi. 297.   [2] Op. cit. p. 125.

lymph. Is it not probable, that, were blood extracted from the cheek, under the influence of the peculiar feeling connected with blushing, it would be found to contain an undue portion of red particles?—that the reverse would happen in blood abstracted from the same surface under the influence of fear; when probably it would be found to abound more in serum. And, in a third instance, as in erysipelas of the face, it would probably be found to contain an excess of fibrin.

In the embryo, a colourless fluid is the first which circulates.[1] Whether, independent of the lymphatics, there is an order of vessels in the adult conveying only a similar fluid, is a subject for inquiry. In the brain, in an instance of jaundice, I have witnessed, on making sections of the organ, points distinct from the ordinary blood-specks, tinged by bile, otherwise colourless, and which, were it not for the adventitious hue, would have been imperceptible. I regret I had not an opportunity of examining the fluid of these puncta with the microscope.[2] The most probable inference seems to me to be, that they were either divided lymphatics, or vessels of the order just alluded to. But, even supposing that no such vessels exist, the lymphatics, under different influences, may pour more or less of their contents into the general circulation, and so affect the quality of the blood.

The quantity of blood abstracted by the leech perhaps may be adduced in support of the argument. It is commonly believed that the blood so abstracted does not differ from that procured by the use of the lancet. According to my observations, it abounds much more in red particles, and very much less in serum and lymph; and, in consequsnce, its character is different—coagulating much less firmly, or often not at all, exhibiting merely a grumous appearance, of unusually high specific gravity, and when evaporated to dryness, affording a larger proportion than common of solid matter. One leech, which I tried in Malta, in May, 1830, gorged with blood, was of specific gravity 1087; cut in two and deprived of blood, one moiety was of specific gravity 1062; the other of specific gravity 1054; the latter was the caudal portion. This leech, distended with blood, weighed 161·4 grs. Another, smaller, which distended with blood weighed only 18·4 grs. was, with the blood, of

[1] This fact appears to have been discovered, if I may say so, at least three different times; by Glisson, nearly two centuries ago,—more recently by John Hunter,—and very lately again by MM. Delpech and Coste. Glisson, in his Treatise on the Anatomy of the Liver, published in 1564, says, "Materia prima, è qua partes fiunt, non est *sanguis*, sed *succus* quidam *liquidus*, albumini ovorum non absimilis, unde pullus efformatur."—*Anatomia Hepatis*, p. 495. Amstel. 1659. Needham, a disciple of Glisson, is even more explicit on the subject: in his treatise "*De Formato Fœtu*," he speaks of a nutritive fluid, which "per venarum oscula nondum rubentia facile recipiatur," p. 83.

[2] Since the above was written, another similar opportunity has occurred; the yellow fluid of the puncta, under the microscope, was found to contain a very few blood particles: it was probably, therefore, extremely dilute blood.

specific gravity 1076, and empty, or very nearly so, of specific gravity 1051. Blood abstracted from the arm, from a patient labouring under an inflammatory complaint, of specific gravity 1061, and which after coagulation was slightly buffed, yielded on evaporation to perfect dryness 20·7 per cent. solid matter. Blood abstracted from the same patient from the scrobiculus cordis by a large leech, similarly evaporated, yielded 22·3 per cent. Now is it not probable, that these peculiarities of the blood abstracted by leeches is connected with the size of the aperture, and the effects of the pumping force exercised by the animal? That the latter influence is concerned, at least in part, may, I think, be inferred from the circumstance, that the blood which flows from the wound after the removal of the leech, commonly coagulates more readily and firmly than the blood procured from the leech itself. The blood extracted by the mosquito is again different in character from that by the leech. It coagulates readily, and is not deficient in fibrin.

8.—*On the effect of oxygen and of carbonic acid gas on the blood.*

My brother, the late Sir Humphry Davy, in his Researches on Nitrous Oxide, states that he did not find the coagulation of venous blood affected by either of these gases.[1] According to Sir Charles Scudamore, blood coagulates sooner in oxygen gas than in atmospheric air, and cools slower; and, in carbonic acid gas it coagulates slower, and cools sooner.[2] The experiments which I have made have afforded results similar to those first mentioned. I have not found the coagulation of the blood distinctly accelerated by oxygen or retarded by carbonic acid; nor have I found any wellmarked difference in either, in relation to the time of cooling. And I did not simply expose the *surface* of the blood to the influence of the gases, in the manner in which Sir Charles Scudamore made his experiments, but I agitated the blood in each gas. Of the oxygen, there did not appear to be any absorption; if there were any, it must have been slight, which the form of the experiment was not fitted to detect. Of the carbonic acid gas there was a very considerable absorption, equal in volume to that of the blood used, or very nearly so. I have also tried the effect of water strongly impregnated with carbonic acid gas on the blood, and of oxygen gas on the crassamentum of the blood, and the results have been similar. The temperature of the cold crassamentum was not raised, and the coagulation of the fluid blood was not prevented.

Merely hypothetically considered, I do not comprehend how either oxygen, hydrogen, or azote, or any other gas which is not absorbed rapidly by the blood, can materially affect the coagulation of this fluid, or can be the means of producing a sensible evolution of caloric, at least by the simple contact of surfaces. I would re-

[1] Op. cit. p. 381.   [2] Essay on the Blood, pp. 59, 61.

mark, however, that I do not consider the negative results of my experiments with oxygen in the slightest degree hostile to that view of the generation of animal heat, in which the chief source of it is referred to the lungs. I need not say, that there is an immeasurable difference between the natural process of respiration and the rude experiments just now alluded to, whether we consider the apparatus, the material, or the situation, and more particularly when we consider that in one instance vitality is present, and in the other that life is extinct, and the agents are merely chemical and dead.

I must confess, too, that, viewed hypothetically, I am equally at a loss to conceive how carbonic acid gas should retard the coagulation of the blood. If it were true that blood abounds in carbonic acid gas, and the coagulation of the blood depends on the evolution of carbonic acid gas, then, in the act of coagulating, it ought not to absorb an additional quantity of this acid gas; or if it did not absorb it, it ought not to coagulate; and indeed it ought not to coagulate, though it did not absorb it, so long as it was in an atmosphere of carbonic acid gas, and so long as it retained the proportion of gas supposed to belong to it. Were there not a consistency in nature, there could be no science. It is not a contradiction that the blood, a fluid not saturated with carbonic acid, capable of combining with a fresh portion, should contain free carbonic acid, and be capable of giving it off. I speak of the blood out of the body, as of a bone or muscle, detached from the animal, deprived of life, and subject to the same laws as ordinary dead matter. Farther on, I shall recur to the important points—the carbonic acid in the blood, and the absorption of oxygen by the blood, with which are involved so much of physiological doctrine.

9.—On the effect of removing atmospheric pressure from the blood, and of excluding atmospheric air.

According to Sir Charles Scudamore, blood coagulates in vacuo more rapidly than in the open air, and becomes of a considerably darker colour.[1] I have tried this experiment repeatedly, and the result apparently has been negative. The blood which coagulated under the exhausted receiver was of the same colour as some which, for the sake of comparison, was left exposed to the atmosphere, and its rate of coagulating was much the same—certainly not quicker—rather slower. On account of the interest of the subject, I shall describe two or three experiments, from which, and from others, I made these deductions in January, 1828, at Corfu.

Put under the receiver of an air pump about an ounce of venous blood, collected as it flowed from the arm in a cylindrical glass vessel, and immediately exhausted the receiver. Until the exhaustion was nearly complete, the blood was perfectly tranquil;

[1] Op. cit. p. 22.

suddenly it boiled up, almost in the manner of an explosion, and continued bubbling during half a minute at least.   On the admission of air into the receiver, all the bubbles instantly subsided. The blood was not coagulated; it coagulated slower than a portion set by in the open air for comparison.

A few days after I repeated this experiment, with this difference— the air of the receiver was slowly exhausted to allow the blood to cool below the boiling point of water in vacuo.   Now, there was no ebullition, no effervescence, no disengagement of air, excepting of two or three adhering globules.   When taken out, this portion of blood had just begun to coagulate; another portion, left exposed to the atmosphere, was coagulated firmly.   The colour of each was similar.

Repeated the experiment; taking precautions to prevent, as much as possible, the adhesion of air to the sides of the glass, by careful cleaning, and wiping, and rinsing it out, previous to the admission of the blood, with distilled water, which had been purged of air.   In this instance, no bubbles of gas appeared in the blood on exhaustion, and the coagulation took place a little later than in another portion of blood exposed to the air.   There was no difference of colour between them.

In the paper in which the general results of these experiments were given, and which was published in the Edinburgh Medical and Surgical Journal, vol. xxx, for 1828—I further stated that I found no material effect produced on the coagulation of the blood by receiving it as it flowed from the arm into oil, which had been previously deprived of air by the air-pump, whether under the influence of atmospheric pressure in the open air, or excluded from it under an exhausted receiver.

Turning back to the notes of these experiments, I find it remarked, that the blood coagulated slower under the oil (whether under pressure or not) than when exposed to the air;—and that, in the former, a much thicker buffy coat formed than on the latter. This result accords with others which I had obtained several years previously in Ceylon; and also with the results of the later experiments of Dr. Benjamin Babington, on the effects of oil.[1]   Probably it is, in part, connected with the cooling effects of the oil on the blood in passing through it; and, in part, to the exclusion of air.

10.—On the effect of water, milk, urine, and bile, on the coagulation of the blood.

It has been remarked by Dr. Crawford, in his treatise on Animal Heat,[2] that, by the dilution of blood with water, in the proportion of twelve parts of the latter to one of the former, its coagulation is

[1] Med.-Chirurg. Trans. xvi. 298.
[2] Experiments and Observations on Animal Heat, p. 278, 2d edition.

retarded for several hours.   In one instance, in which I made this experiment, at the end of two hours the diluted blood appeared to be liquid; but, when thrown upon a filter, this appearance proved to be deceptive; the fibrin had coagulated, and it was collected on the filter in the form of a very tender gelatinous mass.   From this, and from other experiments, I am inclined to infer, as might be expected, *à priori*, that dilution, by removing the particles of fibrin to a greater distance from each other, and by suddenly cooling the blood, does retard its coagulation—but not so long as Dr. Crawford conceived, who formed his opinion on the subject merely from the appearance of the blood.

Milk, urine, and bile, I find, also retard the coagulation of the blood, and more so even than water.   The urine and bile used were human—the milk, cow's.

The results of the experiment with the urine are, perhaps, not undeserving of more particular notice.   About two ounces of blood were mixed, as it flowed from the arm, with four ounces of urine, containing, perhaps, rather a larger proportion than usual of urea. After about an hour and half the blood had gelatinised.   After twenty-four hours, it had coagulated pretty firmly; the coagulum was much contracted and slightly cupped, and covered with a thick buffy coat.   After forty-eight hours, the coagulum was still more contracted, and it had acquired a globular form, with a little con- cavity on its upper surface.   The urine was only just perceptibly coloured by the colouring matter of the blood.   I may here inci- dentally remark, that this fluid is particularly well adapted as a medium for displaying the phenomena of the coagulation of the blood, from its retarding the effect, and yet not diminishing it—and from its not dissolving the colouring matter of the red particles.   It enables one to determine, in a satisfactory manner, especially with the aid of sections, that the phenomena of the buffy coat, and the cupping of the blood, are owing to an unequal distribution of the fibrin; and that, when they occur, there is an excess of fibrin at and towards the surface of the crassamentum, which, it may be in- ferred, was pressed up by the red particles in descending; and a deficiency of it at the bottom, and towards it, in regular gradation; and that the buffy coat is cupped in consequence of the unusual contraction of the fibrin, and its accumulation at the circumference.

11.—*On the coagulation of serum.*

It has been supposed by Mr. Brande, and after him, it has been asserted by several authors, that the liquidity of the serum of the blood is owing to the presence of an alkali—and its coagulation, under certain circumstances, to the abstraction or neutralisation of this alkali.[1]   Dr. Bostock has objected to this view, on the ground

[1] Whether the property of serum was known to the ancients, or, indeed, whether they were acquainted with the fluid itself, is, by some, thought

of the minute portion of alkali existing in the blood—and that this alkali may be neutralised—and yet the serum remain liquid. He is of opinion that the effect of coagulation, in the instance of serum, is one *sui generis*.[1]

The experiments which I have made on serum, have afforded results in favour of this latter view. I shall briefly describe them—preliminary to others on the coagulation of the blood.

The following acids, I find, do not coagulate serum, when cold, namely—distilled vinegar, strong acetic acid, citric acid, boracic acid; the tartaric and oxalic acids, and the aqueous solution of sulphurous acid. I shall notice, in detail, a few of the experiments.

In two instances of the trial of citric acid, it was added in moderate excess to the serum, its crystals having been previously reduced to the state of fine powder. On agitation, the acid was dissolved, and some carbonic acid gas was disengaged. The serum acquired a slightly opalescent hue, and its fluidity, perhaps, was in a slight degree diminished. The serum used was that of the blood of the sheep, which does not coagulate at 160°.[2] It was immersed

doubtful, so imperfect is the history of physical science; moreover, it is even doubtful at what time, and by whom, the coagulability of serum, by heat, was first ascertained. Some writers, as Haller, refer, if not the discovery, at least the first mention of this quality, to Harvey;—others, as Dr. Bostock, consider Harvey's notice of it obscure—and seem to be of opinion that Lower was the first who clearly described it. Lower, in his account of the *liquor pericardii*, remarks,—"Porro humorem istum non merè excrementitium aut instar roris stillatitii aqueum, sed seri potius nutritii è sanguine promanantis, partem esse, exinde constat, quòd ignis calori vel paululum admotus, non aliter quàm serum sanguini post venæ sectionem innatans, aut lympha è glandulis secreta in gelatinam albam incrassatur: qualum quidem consistentiam nec sudor neque urina qualicunque coctione acquirit, sed vel omnino in auras exhalat, aut in sabulum induratur."—(Tract. de Corde, 1669, p. 6.) The author here speaks of the property as if well known to belong to the serum of the blood; and, I apprehend, the same inference may be made from certain remarks of Harvey, in connection with coagulable lymph,—his *mucago* "si saniem illam a grumo separatam et effusam leni foco decoxeris, eandem brevi in mucaginem hanc mutatum iri conspicies. Indicio manifesto, aquam illam sive saniem, a sanguine in pelvi separatam, urinæ forsan materiam aliquam, non autem urinam ipsam esse; licet colore et consistentia talis videatur. Quippe urina coctione non densatur in fibrosam mucaginem, sed potius in lixivium: aquosa autem sive saniosa hæc pars, aliquandiu leviter cocta, in mucaginem innatantem abit: quemadmodum, vice versa, mucago isthæc, corruptione recrudescens, in saiem colliquatione revertitur."—Op. Omn. p. 406. How slow was the progress of animal chemistry, before it was methodically cultivated! Between the time of Aristotle and Harvey, no positive advance appears to have been made; and the former, probably, was acquainted with the property of serum in question! it may be inferred from some expressions which he uses.

[1] Elements of Physiol. vol. i. p. 476.

[2] The point of coagulation of the serum of the blood of different animals is not the same; even amongst the mammalia I have observed a variation of temperature of 10° in this respect. The serum, the subject of the experiment, coagulated at 168°.

in a glass tube in water of this temperature; it remained transparent, and acquired the consistence of thin jelly.

The same acid was next tried in great excess—more was added to the serum than it was capable of dissolving. The results were similar, with this difference—that the jellying of the serum was prevented by heat—even at the boiling temperature of water. The effects of the oxalic and tartaric acids were analogous.

Strong acetic acid, in great excess, added to the same serum, did not coagulate it—some gas was disengaged, and, perhaps, the fluidity of the serum was slightly impaired, but of this I was not quite certain. The mixture, placed in water of 160°, firmly coagulated, and the coagulum was opaque.[1]

A solution, in water, of the sulphurous acid, of moderate strength, appeared to have no effect mixed with serum. When, however, the acid gas was condensed in serum, there was a decided effect, a coagulation was occasioned—a transparent jelly formed—which appeared to be quite insoluble in water, and not liable to putrefy.

The effects of the mineral acids—the muriatic, nitric, and sulphuric, vary with their degree of solution. Equal parts of muriatic acid and water, coagulate serum as readily as the strong acid; but, when diluted with six parts of water, the effect was scarcely visible. Nitric acid, largely diluted, has a distinct effect; thus, one part of this acid to thirty parts of water, had rather more visible effect than the muriatic with six of water; with twenty of water, the serum was rendered very turbid.

The alkaline earths, lime and magnesia, have, apparently, no effect on serum. Baryta, mixed with serum, formed with it a firm semi-transparent jelly. A few hours were required for the combination to take place. In the instance of lime and magnesia, no change was perceived either immediately or after many hours.

Of the salts, some, as the sulphate of copper, the sulphate of zinc, coagulated serum, or occasioned a precipitation of its albumen, and even when extremely diluted. The super-tartrate of potash, in solution, rendered it turbid. Others, as nitrate of barytes, oxalate of ammonia, borax, triple prussiate of potash, chlorate of potash— the neutral salts—did not disturb its transparency.

Nitrate of silver unites with the albumen of the serum, which it precipitates in the form of an insoluble compound; but, though insoluble in water, it is readily soluble in acetic acid; and, it is not a little remarkable, that its solution in this acid is not precipitated by common salt.

A strong aqueous solution of chlorine did not coagulate serum.

Calf's rennet was tried on it, both cold and warm, without effect.

[1] Vinegar, as it is well known, coagulates milk. The effect of a few drops of strong acetic acid is immediate, even unassisted by heat, presenting a remarkable contrast with the effect of the other vegetable acids mentioned above, which, even when in large excess, and boiled, did not coagulate milk.

12.— *On the effect of substances on the blood, which do not coagulate serum.*

According to their effects, as I have ascertained by experiment, these substances may be divided into three classes; viz. 1st, those which do not retard distinctly, or distinctly accelerate the coagulation of the blood; 2d, those which retard or prevent its coagulation; and 3d, those which accelerate its coagulation, or otherwise alter it.

(1.)— *Of those substances which do not distinctly retard or accelerate coagulation.*

The following substances, which I have tried, may be enumerated as belonging to this class, viz. chloride of silver, protoxide and peroxide of antimony, red precipitate, sulphur, iodine, hydrocyanic acid, carbonate of lime, starch, sugar, camphor, sulphuric æther, oil of peppermint, cantharides, oil of turpentine, hydroboracic acid, white oxide of arsenic, and gum-arabic.

In each instance, there was some slight peculiarity in the result, as in relation to the colour which the crassamentum exhibited, or its degree of contraction or consistency. I shall notice a few of them, those which were best marked.

Sugar, refined in powder, added to the blood the instant it was drawn, did not appear to retard coagulation; the coagulum was very soft, and bright red. After twenty-four hours very little serum had separated, and there was scarcely any perceptible contraction of the clot; the fibrin collected by means of a linen filter, gentle pressure being used, was very soft. The fluid part, obtained by filtration, did not jelly when diluted with water. The experiment was made in Corfu, in November, when the temperature of the air of the room was between $50°$ and $60°$. After a week, there were no signs of putrefaction, either in the diluted or undiluted mixture; both had a very slight smell of vinegar.

The effect of the white oxide of arsenic on the blood differed but little from that of sugar; it did not appear either to retard or accelerate its coagulation. The coagulum, however, after twenty-four hours, was comparatively soft, having scarcely perceptibly contracted, and no separation of serum had taken place. The surface was unusually fluid, and also the bottom of the coagulum in contact with the principal part of the oxide. This experiment was made in England, in January, on the blood of a young man, which was healthy in appearance. It appeared to prevent putrefaction.

Gum arabic, added in the state of fine powder, did not appear to retard the coagulation of blood, excepting, perhaps, in a very slight degree. The coagulum formed was very soft, and it contracted

very little. Mucilage of gum arabic, retarded it about twenty-four hours.

Oil of turpentine, also, perhaps retarded the coagulation of the blood in a very slight degree. The effect on the colour of the blood was more distinct ; the oil changed it to a brownish hue.

Cantharides in powder did not appear to retard coagulation ; the crassamentum remained soft, and not contracted.

Red precipitate neither retarded coagulation, or the contraction of the crassamentum when formed, nor consequently the separation of the serum.

Iodine did not appear to retard or accelerate coagulation. Eight grains of it were added to an ounce and a half of blood. It rendered the blood brownish.

(2.)—*Of those substances which retard or prevent coagulation.*

The following substances, which I have tried, belong to this class ; viz. rhubarb, ipecacuanha, cinchona, columba, myrrh, catechu, jalap, the extracts prepared *in vacuo* of belladonna, digitalis, aconite, conium and sarsaparilla, the common extract of cinchona, magnesia, carbonate of magnesia, sulphate of zinc, tartarized antimony, supertartrate of potash, triple prussiate of potash, muriate of barytes, nitrate of barytes, borax, common salt, nitre, sal-ammoniac, the sesquicarbonate of ammonia, the carbonate of soda, sulphate of ammonia, aqua ammoniæ, aqua potassæ, distilled vinegar.

The effects of the substances arranged under this second class, individually considered, are still more various than those of the first. Most of them produced a thickening, and increased the viscidity of the blood. The former was especially the effect of the vegetable substances, and the latter of some of the salts, particularly borax and carbonate of soda. Very few of the substances tried had a contrary effect ; it was well marked only in a few instances, as in those of the triple prussiate of potash, cream of tartar, tartarized antimony, magnesia, and carbonate of magnesia, with each of which the red particles rapidly subsided ; no coagulation took place, and the fibrin remained completely dissolved in the serum. In some instances, and that the majority, owing to the thickening of the blood, or the increased viscidity imparted, the red particles were kept suspended. In a very few, as in those of distilled vinegar, aqua ammonia, and aqua potassæ, they were dissolved. The difference of effect, as to change of colour, was very various indeed ; most commonly the colour was darkened—in a very few instances it was rendered of a brighter and lighter red. This latter effect was most distinct in the instances of nitre, common salt, and borax ; it was never permanent. On dilution with water, almost in every instance, the influence of the foreign substance ceased, and the blood coagulated ; and, I believe, it would have done so in every instance, with the exception of vinegar, and aqua ammoniæ,

and potassæ, and apparently of magnesia, were the due proportion of water known which should have been added. Mr. Hewson, I believe, was the first who noticed this suspension of the power of coagulation and the restoration of it by the addition of water. His observations were confined to the neutral salts.[1] The notices which he gives of his experiments are very brief; he does not allude to the length of time the effect of suspension of coagulation may be continued, nor does he appear to have been aware, that conjointly with this the tendency to putrefaction is arrested. From what I have witnessed, I believe that blood may be kept liquid or viscid for days, or even weeks, without losing the power of coagulating, and the fibrin of contracting, when properly diluted with water. In the one state, I find it resists putrefaction; in the other it readily yields to it. I have said, that vinegar, and ammonia, and aqua potassæ, are exceptions in this class of substances, differing from them in preventing the coagulation of the blood, however carefully diluted with water, and that magnesia is apparently an exception also. The vinegar and the aqua ammoniæ, and potassæ clearly keep liquid, and that permanently, the fibrin of the blood, as well as dissolve the colouring matter of its red particles.[2] The appearance of magnesia being an exception also, I have found is a deception, and chiefly in consequence of the circumstance, that on dilution the coagulable lymph does not form a glutinous mass, but is precipitated in flocculi.

From these general remarks, I shall proceed now to particular instances, noticing only those in which the effects were best witnessed.

Extract of digitalis prepared *in vacuo.* Previous to mixing it with blood, it was softened with water and made of thin consistence; a dram of water having been added to half that quantity of extract. The blood (about two ounces) as it flowed from the arm,

[1] The following note appears to contain the results of Hewson's experiments on this subject—" The salts which keep the blood fluid by itself, and yet allow it afterwards to jelly on being mixed with water, are sal glauberi verus (sulphate of soda), sal digestivus sylvii (chloride of potassium), sal communis, nitrum commune, nitrum cubicum (nitrate of soda), sal diureticus (acetate of potassa), borax, the salt made of vinegar and the fossile alkali, and the salt made with vinegar and chalk." He adds, " the following salts, likewise, keep the blood fluid, but do not allow it to jelly when mixed with water: tartarus vitriolatus (sulphate of potash), sal epsomensis, sal ammoniacus communis, sal ammoniacus nitrosus, sal rupillensis (tartrate of potassa and soda)."—Experimental Enquiry, foot-note, p. 13.

[2] Strong acetic acid renders blood dark and viscid; in one instance in which a dram of acid was added to three drams of calf's blood, the mixture, though viscid, admitted of being poured from one vessel into another. The red particles did not subside; under the microscope, their appearance was distinct, perhaps a little wrinkled at their edges, and perhaps a little expanded. When a drop of this viscid blood was mixed with a dram of strong acid, from almost black it became of a reddish hue; and the red particles, it may be inferred, were dissolved, as under the microscope they were no longer perceptible.

was received into the vessel containing the extract and immediately stirred. The first effect was a thickening of the blood; it acquired the consistence of paste. After twenty-four hours, the appearance was much the same, as it was also after thirty-eight. The following day, the lower part of the mixture was found to be more vis-cid, as from the subsidence of the coagulable lymph. Water was now mixed with it, and the result was that it jellied.

Extract of opium; similarly and in like quantity mixed with water, and afterwards with blood. After two hours the blood was liquid: the red particles had subsided, leaving a stratum of viscid fluid which admitted of being drawn out into threads; after three hours, slight coagulation had taken place; after twenty-four hours, the coagulation was firm, cupped, and exhibited a buffy coat.

Sulphate of zinc. A dram of this salt in powder, was mixed with about an ounce of blood. It thickened immediately but did not coagulate. After twenty-four hours there was no further ap-parent change. On dilution with water, a white pellicle formed suddenly, resembling a false membrane, from the sudden coagula-tion of the fibrin.

Sulphate of ammonia, in aqueous solution. After three hours the blood with which it was mixed was liquid and its colour un-changed. After twenty-four hours, the colouring matter had sub-sided, the supernatant fluid was like serum, it did not jelly.

Magnesia and carbonate of magnesia—15 grains of each in im-palpable powder were mixed with half an ounce of blood. The blood in each instance acquired a more florid hue, remained liquid, and the red particles subsided. On the following day, viz. the 19th January at Corfu, the supernatant fluid had the appearance of serum. Now diluted with water, after twenty-four hours, in the instance of the carbonate it was found to have jellied; and in that of the magnesia, to have afforded a precipitate of lymph in a flaky form.

Cream of tartar: a dram of this salt was added to an ounce of blood. The blood thickened a little and effervesced slightly, and became of a brighter red. After three hours it had not coagulated. After twenty-four hours the red particles had subsided; the super-natant fluid was like serum. On dilution with water it jellied; the coagulum was of a very soft consistence.

Chloride of barium: 12 grains of this salt were mixed with an ounce of blood. After five minutes, it remained liquid; the colour of the blood was perhaps a little brighter. After twenty-four hours, there was a slight subsidence of red particles and separation of fluid. It was a little thicker, but still liquid. With water it jellied.

Nitrate of barytes, mixed with blood in the same quantity, had an effect very similar.

Muriate of ammonia: 20 grains of this salt in powder were mixed with an ounce of blood. The blood was immediately dark-ened. After twenty-four hours, it was of a very dark red, and vis-cid like thick bile. No fibrin could be obtained separate, by passing

it through a linen filter. It had a strong ammoniacal odour. A portion of it diluted with water coagulated pretty firmly. After six days, in November, at Corfu, the diluted blood became putrid. The portion not diluted was free from putridity, and after this interval coagulated with water as in the first instance.

Sesquicarbonate of ammonia: 50 grains of this salt in powder were mixed with three ounces and a half of blood. There was a change of colour as in the preceding, in which the muriate was added. After twenty-four hours, it had separated into two parts, a supernatant orange-yellow fluid, and a viscid dark red sediment. A portion of each diluted with water coagulated; the former firmest. After three days, in the same place and season, the diluted portion was slightly putrid; whilst that not diluted was unchanged, and retained its power of coagulating when mixed with water.

Sesquicarbonate of soda: 50 grains of this salt in powder were mixed with three ounces and a half of blood. The blood immediately became of a brighter red. After twenty-four hours it darkened, became very dark and viscid, in consistence like thick bile. There was only a very slight subsidence of the red particles. On dilution with water, it did not coagulate. The portion diluted after three days, in November, was putrid: the undiluted portion was free from taint.

Common salt: two drams of this salt in powder were mixed with two ounces of blood. Examined a few hours after, the blood was found thickened, but not distinctly coagulated. After forty-eight hours, it was much in the same state. Passed through a linen filter, no fibrin remained. Diluted with about four times its bulk of water, it coagulated and formed a gelatinous mass.

In another experiment, 20 grains of common salt were mixed with an ounce of blood. It prevented coagulation: and the colour of the blood was immediately brightened. After twenty-four hours the blood was found viscid and dark. Filtered through linen, no fibrin was collected. It jellied with water. After six days, in November, the diluted portion had become putrid; the other not, and retained its power of coagulating on admixture with water.

Saltpetre: mixed two drams of this salt with two ounces of blood; after a few hours the blood was found thickened. Passed through a linen filter, a minute portion of very soft fibrin was collected; after forty-eight hours, the filtered portion jellied with water.

In another experiment, 20 grains of this salt were mixed with an ounce of blood; partial coagulation presently took place: after twenty-four hours, a soft crassamentum formed, and pretty much fluid separated. Subjected to filtration, gentle pressure being used, some fibrin of soft consistence was collected. The filtered fluid coagulated weakly on dilution with water. After twenty-four hours, this portion was putrid: the undiluted had no unpleasant smell; but it did not now coagulate with water.

In a third experiment, twenty grains of nitre in powder were mixed with an ounce of blood, previously deprived of its fibrin, by agitation with pieces of lead in a bottle, quite full of blood, and from which in consequence atmospheric air was excluded. The colour of the blood became a little lighter, but rather of a brick-red hue than vermilion. Now on exposure to the air, at the surface it acquired a very florid scarlet hue, very similar to that imparted by oxygen. This thin coloured stratum subsided, and it was succeeded by another and another; till the whole mass exhibited a stratified appearance—a succession of bright and darker lines; analogous to what takes place in the cruor, similarly exposed without the addition of nitre, but less strongly marked.[1]

Borax: fifty grains of this salt were mixed with three ounces and a half of blood; it immediately assumed a brighter red; after twenty-four hours, the blood was liquid; the red particles had completely subsided; the supernatant fluid was of a bright yellow colour, like serum. Both the inferior and the superior portion coagulated pretty firmy on dilution with water. After three days the diluted portion was slightly putrid; the other was free from any unpleasant smell, and retained its power of coagulating; and even after sixteen days it possessed this power. The experiment was made in Corfu, in November.

Triple prussiate of potash : a dram of this salt, in fine powder, was mixed with three ounces and a half of blood; coagulation was prevented; the colour of the blood was a little brightened: the red particles rapidly subsided, and the supernatant fluid was like serum. On dilution with water, the fibrin coagulated very slowly, and the coagulum which formed was of very soft consistence.

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[1] These results accord with those of Haller, as described by him in his Elementa Physiolog. ii. p. 74. After noticing the difference of opinion previously existing on the subject, whether nitre dissolves or coagulates the blood, he says, " A nitro sanguis neque solvitur, neque cogitur, in meis quidem experimentis. Admisto enim nitro laminæ equidem cruoris floridissime rubentes nascuntur, verum eæ, etiam absque nitri ope, pariter ex eodem sanguine coeunt. Color autem vividissime coccineus utique nitro debetur, quo sale non alius lætore purpure sanguinem ditat." Relative to the difference of opinion previously entertained, he observes, " Ceterum, si d' lite illa omnino definire oporteret, crederem nitrum sanguinem potius resolvere, siquidem leviorem reddit, neque mea experimenta penitus repugnant." He adds, " Alii sales medii utcunque similia faciunt. Sal marinus cum sanguine commistus floridum colorem facit, cæterum coagulationem neque valde promovet, neque impedit, nisi motus accesserit, qui etiam solus coagulationem impedivissit." This may be considered as a tolerable summary of what was then known on the subject, coupled with a remark which precedes his observations on nitre. " Ea communis salium mediorem et lixiviosorum natura est, ut fluidum plerumque sanguinem reddant, aut liquidem conservent, colorem vero rubrum aut firment, aut omnino augeant."

(3.)—Of those substances which accelerate coagulation, or other-
wise alter the blood.

The following substances, I find, belong to this class, namely—
the oxalic, tartaric, and citric acids, chlorate of potash, lime, and
calomel. No doubt, the number will augment on farther inquiry.
I shall notice, briefly, the apparent effects of each.

Oxalic acid : fifty grains of this acid, in powder, mixed with two
ounces of blood, immediately coagulated it ; the coagulum was of
a pitch colour, and of firm consistence, and no serum separated
from it. A smaller proportion of the acid rendered the blood thick.

Tartaric acid : thirty grains of this acid, dissolved in two drams
of water, mixed with two ounces of blood, prevented immediate
coagulation : after two hours, the mixture became frothy from the
disengagement of air, thick like tar, dark brown and viscid ; after
twenty-four hours it had lost its viscidity, was firmly coagulated,
but without contraction and separation of serum, and was easily
friable.

Citric acid : thirty grains of this acid, with the same quantity of
saturated aqueous solution, mixed with two ounces of sheep's
blood, slightly retarded its coagulation ; in half an hour the blood
coagulated. The coagulum was firm and tough, and of a brown-
ish hue; after twenty-four hours it had not contracted or yielded
serum. A small portion of it, agitated with serum, reddened it. A
portion of a drop of this reddened serum, under the microscope,
was found to contain red particles, little, if at all altered ;—they had
their usual disc-like form.

Lime, mixed with blood, immediately darkens its colour, and
speedily forms with it a tough mass, which, on exposure to the air,
shortly becomes firm and hard.

Chlorate of potash : fifty grains of this salt, in powder, mixed
with an ounce of blood, retarded its coagulation, and brightened
its colour; after twenty-four hours, it was found firmly coagulated
and of the colour of pitch. No serum had separated from the
coagulum ; it contained some air-bubbles and had a peculiar smell.

Calomel : thirty grains of this chloride were added to half an
ounce of blood ; its coagulation was rather accelerated, and the
coagulum was firm.

13.—On the effect of substances on the blood which coagulate se-
rum, or precipitate its albumen.

The following substances come under the above denomination :
the sulphuric, nitric, and muriatic acids ; sulphurous acid, barytes,
alum, nitrate of silver, sulphate of copper, alcohol.

All of these apparently coagulate, both the fibrin and the serum
of the blood. The effect is produced almost instantaneously, ex-
cepting in the instances of sulphurous acid and barytes—and is

generally attended with a change of colour of the red particles. Neither the sulphurous acid gas, nor its aqueous solution, nor barytes, has any perceptible immediate effect, except that of darkening the blood ; but, in a few hours, both the acid and barytes, as might be expected from their operation on serum, render it of firm consistence like strong jelly.

The blood used in all the experiments, the results of which have been given, when not specially mentioned, was venous blood, taken chiefly from men in the prime of life, or but little beyond it, labouring under various diseases. I have not considered it necessary to specify the nature of these diseases, or the peculiarities of the blood in each instance—the object of my inquiry being, on the present occasion, totally different from the investigation of the properties of this fluid in a morbid state. It may be proper, at the same time, to notice, that disease was not lost sight of—and that in such experiments, in which it was desirable to use healthy blood, the blood employed was from men labouring under slight complaints, or just attacked with severe ones, and a portion of it was always set aside, undisturbed, for comparison.

Hewson states that he did not give a detailed account of his experiments on the action of the neutral salts on the blood, because he was of opinion that they were not applicable to medicine. Haller, after passing in review the operation of a considerable number and variety of re-agents, arrived at the same conclusion. The paragraph in which he expresses this opinion, is deserving of attention in the present times ; I shall give it entire.

" Arsenicum, dubium corporis genus, sanguinem cogit[1] rubrumque reddit. Cæterum infinita alia cum variis succis instituta experimentis, apud cl. viros legere oportet, quos excitavi. Obiter antem in iis, quæ repetii, discas, quam parum ad medicamentorum virtutes dignoscendas utilitatis ab his experimentis possis arguere. Arsenicum enim, summum venenum, sanguinem fere ut beneficum illud nitrum mutat. Acetum inter omnia pharmaca tristissimum colorem sanguini inducit, non ideo minus innocuum. Acidi liquores et lixiviosi pariter cogunt."[2]

Reflecting on the results which I have described, and proceeding on the principle of induction, the conclusion of Hewson and Haller, in the present state of our knowledge, appears to be the only ligitimate one. Haller contrasted the effects of white oxide of arsenic and vinegar. Other substances might be brought forward in the same manner—as the hydrocyanic and hydro-boracic acids ; the salts of barytes and strontites, and of lime and magnesia ; the oxalic and the citric acids ; and a large number of the vegetable extracts.

If no relation can be discovered between the operation of a substance on the blood, out of the body, and of the same substance

[1] This effect I have not witnessed, vide p. 54.
[2] Element. Phys. tom. ii. p. 83.

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taken internally, the conclusion above referred to is necessarily strengthened; and it seems unwise to have confidence in any hypothesis, founded on partial relations of the kind. As the subject of the blood, in all its bearings, is one of great obscurity, all speculations concerning it require to be received with much caution and examined with all possible rigour. Adopted, without due and searching scrutiny, hypothesis can hardly fail to exercise a very injurious influence; and to have the contrary tendency, if it stimulate research, and that careful inquiry, by which alone disputed points can be determined.

14.—Some remarks chiefly of a speculative kind relative to the question, Is the blood a living fluid?

By one train of reasoning, the conclusion, that the blood is a living fluid, seems almost demonstrated; whilst by another train of reasoning, the same appears almost doubtful.

Can it be supposed probable, or even possible, that a fluid, itself void of life, can circulate through the body imparting vital energy; deprived of which fluid, every function is interrupted, every organ soon ceases to act, and the part, or parts, or whole, almost immediately die?

Again, is it possible to conceive, that a fluid which belongs only to animals, which is generated in them from its elements, which can exist in them only under ordinary circumstances, changing its character the instant it is abstracted, without absorbing, or evolving any thing sensible or cognisable "in quo (as Harvey forcibly and eloquently states) vegetativæ et sensitivæ operationes primo elucent: cui calor, primarium et immediatum animæ instrumentum, innascitur: qui corporis animæ que commune vinculum est: et quo vehiculo anima omnibus totius corporis partibus influit."[*] Is it possible to conceive that this is a dead fluid?

In opposition to this view, it may be replied; it is only a little more difficult to conceive the blood dead, than the chyle, and the chyle than the chyme. The food is admitted to be dead; yet it is grateful to the palate, and certainly not injurious to the gullet and stomach, and whole *primæ viæ*. From it chyme is formed. May not chyme, in relation to the duodenum, act the part of the food in relation to the stomach, and like the food be dead? Chyle is supposed to be derived, or formed from chyme; and, may not it act on the parts with which it comes in contact, like the food on the stomach, and the chyme on the duodenum, and be considered also dead? And may not the same be said of the blood which is derived from it? In many of their properties, chyme and chyle have a certain resemblance, and chyle and blood a close resemblance. They (chyle and blood) both contain suspended in them solid particles of a definite form; both contain a spontaneously coagulable

* Op. omnia, p. 398.

fluid, and, another fluid ingredient coagulable by heat: they differ chiefly as regards the solid particles—in the one, spherical and colourless, or nearly so; in the other, coloured, and in the instance of the mammalia, of a disc-like form.

Descending in the scale of organisation, the reasoning just employed appears even more conclusive. In certain animals, there appears to be but little difference between the chyme or alimentary fluid and the blood—the former passing into the blood-vessels direct. And in certain other animals, there appears to be no difference— their chyme or alimentary food, being what is commonly called their vital fluid, and distributed by ramifications of the alimentary canal, they having no regular vascular system.

In further prosecution of the latter train of argument, it might be maintained, that many of the reasons which are commonly assigned in favour of the blood not being dead, are applicable only to arterial blood; and so far they may be retorted to prove, that if one kind of blood is alive, the other is dead; and, consequently, that the circulation of the blood may be viewed as a perpetual miracle, in which material particles are without cessation dying and reviving.

Farther, in prosecution of the argument, it may be said, that the blood considered chemically, however extraordinary, is not without analogy, and that some vegetable juices make a near approach to it in their proneness to change, and in the tendency of their elements to enter into new combinations. Witness the juice of the grape and most of the other saccharine vegetable juices, which at a certain temperature, like the blood, can only exist in their proper vessels and receptacles. The instant they are abstracted the process of fermentation begins, and that even in close vessels, and though oxygen be excluded, as it appears to me, I have proved by experiment. Here then, we have an instance of change as great as that which the blood undergoes, when drawn from the body, spontaneously taking place amongst the elements of dead matter. If it be said, that the vegetable juices possess vegetable life, by which they are preserved in their vessels, and that they are to be considered only as dead when expressed, and when in consequence they are liable to fermentation, another instance free from such objection may be substituted, viz. a solution of crystallised sugar, which is subject to the same change on admixture with a little leaven—the leaven itself, it would appear, undergoing no change in the act of exciting fermentation.

The subject may be considered under many other points of view, and arguments for and against the vitality of the blood easily adduced. Perhaps it may not be amiss to prosecute it a little farther, nor uninteresting to those who are fond of such speculative questions.

In support of the vitality of the blood, it may be urged, that one of its ingredients—fibrin, or lymph—when effused, often becomes solid in a very short time, and vascular. When vascular and organised, it may be admitted as alive, even by the casuist; and,

also, when the vessels are forming in it, and the moment before that
event; and so on to the period when it was effused, or prior to that
when it formed a part of the circulating fluid: and, if one part of
that fluid be allowed to possess life, it would be extraordinary in-
deed, if the other parts of it were destitute of life.   By a similar
mode of reasoning, from the changes which take place in the egg,
from the formation of the fœtus, and the influence of the spermatic
fluid—(a fluid derived immediately from the blood)—the same con-
clusion may be drawn.   How difficult to suppose a living fluid a
particle derived from a dead fluid—that that which animates should
be itself destitute of life, or be furnished by one without life.

In opposition, it may be replied, organisation is an effect of life;
but the effect or product is not necessarily living, and a part may
belong to the living body, and yet be without life.   Tartar, a mix-
ture of phosphate of lime and a little carbonate with mucus, desti-
tute of life, occasionally firmly encrusts the teeth.   Dead bone, it
has recently been found, may be united to living bone; a union
may take place by a deposit of new bony matter, connecting the
two, so that, were it not known that the added bone was dead, it
might be inferred to be alive.[1]   Moreover, we know, that foreign
bodies have often continued enveloped in living parts, as a ball in a
limb, as if they were not foreign bodies, occasioning no pain, no
irritation, no morbid action.   Very recently, a patient was admitted
into the general hospital at Fort Pitt, after amputation of his fore-
arm on account of a malignant tumour.   The stump was cica-
trised, but the ligatures employed in securing the vessels had not
come away.   About four months after the operation he died, ex-
hausted from the effects of the malignant disease which had broken
out in the arm and shoulder.   I examined the ligatures; they had
been perfectly preserved, enclosed in the cicatrix, as if they had
been living parts, indicating demonstratively that lymph had been
thrown out around, and that it had adhered to them.

The hair, nail, cuticle, and enamel of the teeth are products from
living parts, and all of them may be considered as organised; but
they are not generally considered as alive, and yet how little do
they differ in want of indication of vitality from the lens of the eye,
from bone, cartilage, tendon, and even the blood-vessels themselves.
It is not necessary to insinuate, if the former are dead, why not the
latter?   But, it might be urged rather, if the latter possess life, why
not the former?   Is it not as reasonable to suppose hair, or the cuti-
cle alive, as the substance of an adhesion formed of effused coagu-
lable lymph?

Or, taking a wider range of argument, the mystery of the subject
may be dwelt on.   It may be asked, is it more mysterious or inex-
plicable that a dead fluid should circulate in living vessels, than

[1] Vide Transactions of Medico-Chirurgical Society of London, vol. xix.,
for a notice of this curious fact by Mr. Gulliver; and also the 3d Fasciculus
of Drawings, selected from the Museum of Morbid Anatomy, at Fort Pitt.

that dead parts should be attached to living parts?   Is it more mys-
terious, that one kind of fluid, the spermatic, should excite an ovum
to change, developement, and growth, than another kind of fluid,
milk or broth (both having been previously subjected to the boiling
temperature) should nourish an animal, supplying new living par-
ticles to the body; in excess occasioning growth in infancy; in de-
ficiency, decay in old age?   The serum of the blood, after coagu-
lation, apart from the clot, may be injected into the vessels of a
living animal, and will circulate without deleterious effects.   The
crnor, after the separation and coagulation of the fibrin, after having
been out of the body several hours, may also be injected into the
blood-vessels, and resume its place in the circulation, without inju-
rious effects;[1] if dead, acting as if alive, and consequently in a man-
ner confounding the difference.

I am tempted to give another form of argument, because con-
nected with a curious fact.

Against the vitality of the blood, it may be said, its characteristic
properties may be preserved out of the body, under some circum-
stances most unfavourable to life.   Thus by the addition of a neu-
tral salt, it may be kept liquid, and on dilution with water, after
many days it will coagulate.   It may be kept liquid too by a tem-
perature of 32°, I know not how long; and on raising the tempera-
ture, it will coagulate.   Even freezing the blood, by reducing its
temperature below 32° does not destroy its peculiar properties.
When exposed frozen to a gradually increasing temperature, it
liquefies, and the liquid presently coagulates.   How, it may be
asked, are these phenomena compatible with its being a living
fluid?   Can it retain life when frozen?   If not, and yet become
liquid and after that coagulate, its liquidity and coagulation are
independent of life.

To these arguments, again, it may be replied, it does not neces-
sarily follow that a neutral salt, or a temperature of 32°, or the
freezing of blood deprives it of vitality.   Certain seeds will bear a
very low temperature, much below the freezing point of water, and
germinate afterwards.   Some parts of warm-blooded animals may
be frost-bitten, and yet not destroyed. The common medicinal leech
may be frozen and kept frozen for a length of time, and yet not
killed.   In the winter of 1827, I had proof of this, when going, in
company with my brother, the late Sir Humphrey Davy, from
England, through France, into Italy, by Mount Cenis.   It may be
proper to give some details respecting a fact, which to the best of
my knowledge is new.   For about a month, the cold was very
severe, so much so, that some leeches which were kept inside the
carriage in a bottle of water, were included in a solid mass of ice,

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[1] Provided it is not from a different class, or even species, of animals; it
the former, a fatal effect is rapid; if the latter, the effect is injurious. often
mortal.   Vide Annales de Chemie et Physique, tom. xviii. p. 295, for the
results of the researches of M. M. Prevost and Dumas on this subject.

from London nearly to Modena; and, it must be inferred, were frozen, as the temperature was often below the freezing point, and in passing the Alps, only a few degrees above zero, of Fahrenheit's scale; notwithstanding, when the ice was gradually thawed, the leeches all, with the exception one, revived, and after this lived a considerable time.[1]

It may indeed be said, however improbably, all the circumstances considered, that the leeches were merely included in the ice in this instance, and not actually frozen. To ascertain the truth of such a notion, I have tried the effect of artificial cold, by means of freezing mixtures, on these animals, by which they have been rendered rigid and hard, so that they were doubtlessly frozen; and yet, when thawed they revived; but with the exception of one, the death of all of them speedily followed. This result is the more remarkable, as the experiment was made during the height of summer in Malta, and with leeches suddenly taken from water almost tepid, and plunged into a freezing mixture, and when rigid were as suddenly plunged into the tepid water. Now, to return to the argument, if an animal, or even a part of an animal, can be frozen and not deprived of life—why may not the same happen to a fluid? Why may not its animation, in the same way, be suspended and restored?

The blood, after death, I have often found liquid, and that many hours after death, when cold, but still retaining its power of coagulating. What is to be hence inferred? Does it not follow either that the liquid blood, though coagulable, is dead, and consequently, that coagulability is not a vital property? or that this blood possesses life after the death of the body—after the muscular fibre has lost its irritability, and the limbs have become rigid, and even partial decomposition has commenced?

It would require little ingenuity to extend and vary these speculations; but I apprehend, the farther they are extended, the more difficult it would be to decide and come to a positive and satisfactory conclusion. The question is almost metaphysical. Whichever conclusion is adopted—whether the blood be a living fluid—or merely the product of life and pabulum, without vitality—not much stress need be laid on it, or value attached to it. Our great object should be to investigate its properties by careful observation and accurate experiments, by which method alone our knowledge of it can be extended, and, if possible, our doubts removed. *Anticipatio mentis*, as emphatically called by Bacon, is admirably adapted to

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[1] The fact is thus described by my brother, in his Journal: "On the 14th (March, 1837), I made use of two leeches, which bit immediately, and performed their office well; yet they had been frozen from January 23d to Feb. 22d, and had been sometimes exposed (as on Mount Cenis) for more than fifteen hours to a temperature below 10° Fahrenheit. They were thawed very gradually, and appeared dead when first thawed, but recovered in some hours in a warm room. They had all (they were twelve) adhered together in a sort of ball, and were precisely in the centre of the bottle, at the greatest distance from the cooling causes."

create doubt, but not to remove it. It is chiefly useful, when it leads to honest and zealous inquiry, by which alone truth, the *interpretatio naturæ* of the same author, can be attained.

15.—*Observations relative to the question—Is the appearance of the blood abstracted, as a remedial means, a just criterion in considering the propriety of repeating the operation of bloodletting?*

It is of importance, in so momentous a subject as the abstraction of blood, that we should come to some decisive conclusion relative to the above question—and, whether negative or affirmative, that it should be well founded on facts, and not matter of opinion.

Let us first consider the appearances and qualities of the blood, which are commonly supposed to be indicative of inflammation, and to warrant rather than forbid further blood-letting. They are chiefly the following:—an unusual degree of fluidity of the blood, the instant it is drawn; unusual slowness in coagulating; and, when coagulated, being covered with a buffy coat and cupped.

Relative to these appearances and qualities of blood, experience seems to show, that they are met with in the majority of cases of local inflammation, but with shades, differences, and exceptions, involving much difficulty and perplexity.

1. When the inflammation is violent, rapidly running on to suppuration, and very extensive, as attacking at the same time more than one texture—or only the same texture, but in different organs —the blood drawn, I have observed, is neither buffed nor cupped. This I have witnessed most strikingly, in cases of peritoneal inflammation, either pure or complicated, with inflammation of the mucous coat of the intestines, or with diffuse cellular inflammation.

2. In diffuse cellular inflammation, I have often noticed that the blood coagulated rapidly, as rapidly as in health, and yet being unusually liquid, exhibited a slight buffy coat—provided, as has been already mentioned, the vessel used for holding it was filled in a few seconds, and instantly put aside and allowed to remain undisturbed.

3. In ordinary cases of inflammation, as of the pleuræ and lungs, the blood drawn at the commencement of the disease is, occasionally, not buffed nor cupped ; but, on repeating the operation the following day, the blood exhibits both these qualities.

4. In cases of inflammation of the mucous coat, whether of the air-passages, or of the alimentary canal, the blood drawn sometimes shows the appearances and qualities enumerated, and sometimes not.

5. Experience does not seem to have established, even generally, any relation in point of degree, between these appearances and qualities and the intensity of the inflammation. Sometimes the buffy coat on the blood is very thick, and the crassamentum is much contracted, and the symptoms of inflammation are not violent—and

the recovery is not long protracted; and sometimes the reverse of this occurs.

6. In a large proportion of fatal cases, fibrinous concretions, or polypi, as they were formerly called, corresponding to the buffy coat on the blood drawn, are found in the heart and great vessels, and, as well as I can judge from my experience, as often when the lancet has been freely used, or only moderately, or not at all.

The consideration of such facts, I must confess, deprives me of confidence in these appearances, as indications for practice and the repetition of blood-letting, and induces me to conclude, that as a criterion in this way, they cannot be pronounced just or safe.

Let us next consider the appearances and qualities of the blood, commonly supposed to be connected with a state of the system, as it were, the opposite of the inflammatory, and not to warrant further blood-letting. They are chiefly, as it is supposed, a very soft crassamentum, very little, if at all, contracted—or the blood remaining liquid—or the proportion of crassamentum to the serum being unusually small.

So far as I am able to judge, these appearances and qualities of the blood are not proved, by experience, to be connected with the state of the system supposed.

1. In the remittent fever of hot climates, and in cholera morbus, both the common kind and the epidemic, the crassamentum of the blood drawn is generally softer than natural, and little, if at all, contracted; and yet, blood-letting, in these diseases, is not generally injurious—it is often beneficial—and occasionally beneficial even when repeated.

2. Blood, without fibrin, in any disease, is very uncommon. I have witnessed it in extreme degree, only in cases of pulmonary ecchymosis or pulmonary apoplexy, as that lesion has recently been called—and *that* after death, in the cavities of the heart and vessels —but so soon after death, that it may be taken for granted, that it was not a *post mortem* change—that it existed previously, and was probably connected with, and may have been partly the cause of the fatal effusion. In such cases, no practitioner, I suppose, would have hesitated, if called in in time, in abstracting blood, especially taking into consideration the plethoric habits of the individuals to whose cases I allude, and their previous apparent robust health.

In a less degree, I have witnessed the same state of blood in cases of cynanche—and in one in particular, after death, which made a strong impression on my mind. This I shall briefly notice, as it appears to me instructive, bearing on the points in question. A young healthy man, previously liable to cynanche tonsillaris, was attacked with sore throat. On admission the same day into hospital, he complained of little excepting some difficulty in opening his mouth and swallowing. An emetic was administered and a purgative. At the evening visit, he appeared to be easier; the pulse 70, soft, and rather feeble; no pyrexia. The medical officer left him without the

slightest apprehension. At three o'clock, the following morning, he was called to see his patient and found him dead. According to report he fell early asleep; awoke a little before three in a state of confused alarm and of breathlessness, and expired before aid could be afforded. I was present at the *post mortem* inspection : œdema of the glottis, and epiglottis, and of one of the sacculi laryngis, accompanied with the same state of the uvula, and some effusin in the course of the great vessels, sufficiently accounted for the fatal event. The probability is, that had he been blooded, and largely, his life might have been saved; yet, the blood, after death, was almost destitute of fibrin ; no fibrin had been poured out— merely serum; nor did the state of the pulse or of the skin seem to indicate the propriety of blood-letting.

Of the efficacy of blood-letting, in cases of this description, I may mention an instance, analogous to the preceding, excepting in its termination. A patient, with cynanche, in the general hospital at Fort Pitt, suddenly experienced a feeling of suffocation. I was, fortunately, in the ward at the time with an assistant. A vein in each arm was opened as speedily as possible, and a considerable quantity of blood abstracted. As the blood flowed, the dreadful sensation of suffocation subsided; perfect relief was afforded; and he soon recovered completely; but it is worthy of remark that the large blood-letting was followed by violent delirium of short duration.

3. The proportion of the crassamentum to the serum being small, may often be witnessed in acute diseases in their advanced stage, or in acute diseases supervening on chronic of long duration—or occurring in persons of great delicacy of constitution, of feeble health, or of valetudinary habits. In such cases, no judicious practitioner would think of using the lancet, unless he considered it urgently necessary; and then surely he would not be hindered from using it, even if certain of the blood being below par, in respect to the proportion of the crassamentum it might yield.

Lastly, there is another, and extensive and most important class of diseases (were a classification formed according to the appearances and qualities of the blood) in which the blood, as far as we have learned from experience, is not apparently altered, as in the continued fever of summer, especially in the south of Europe, differing but little from the ephemera—as in the early stage of synochus—as in apoplexy, and tetanus, and many of the diseases strictly belonging to the neuroses, in the treatment of which blood-letting is often useful and sometimes indispensable—and as in aneurism. These considerations tend to support the former conclusion. It appears to me, that the more experience medical practitioners have, the less confidence they place in the appearances of the blood, in forming their opinion on the nature of a disease and on the mode of treating it. What were the sentiments of the late learned and experienced Dr. Heberden on this matter? He says, "the more we know of the human body, the more reason we find to believe, that

the seat of diseases is not to be sought for in the blood, to the sensi-
ble qualities of which they seem to have very little relation; and
though it be supposed to hold in all maladies, yet in reality, it is
but in very few, that the blood affords the practitioner any useful
information." This is called by Sir Charles Scudamore, in his
Essay on the Blood, a remarkable passage. In the state of know-
ledge of the blood, at the time it was written, there can be no doubt,
that the practical conclusion with which it ends was perfectly cor-
rect; and I fear it is still so, and believe that it is, though our know-
ledge of this fluid, since then, has been increased; and that it is so,
even more strictly than expressed, even in all diseases, and without
the exception of " very few." Is there, I would ask, any one dis-
ease, in the considering and management of which, the skilful prac-
titioner considers himself under the necessity of examining the
blood? I am not aware that there is one such. In cases of pneu-
monia and pleurisy, it was, and is perhaps still usual for the prac-
titioner to look at the blood. Suppose the blood is not buffed, and
in a few hours the symptoms generally indicate the propriety of
abstracting more blood, will the practitioner hesitate to abstract it,
merely from the absence of the buffy coat on the blood just drawn?
Suppose the blood abstracted in a case of pulmonary inflammation,
supervening on tubercular phthisis, is strongly buffed, and the symp-
toms continue but little if at all mitigated, will the practitioner, on
account of the state of the blood, repeat the operation of blood-let-
ting? Most probably he will not. He will consider rather the
peculiar state of his patient, and endeavour to subdue the inflamma-
tion by other means.

Though I believe the time is not yet arrived, that in relation to
practice, we can derive advantage from inspecting the appearances
of the blood, yet it is to be hoped that it will arrive; and that it will
come, seems probable, if the subject is carefully investigated in a
strict scientific manner by those who are unprejudiced, and are
competent to engage in the investigation. Dr. Heberden, in the
passage quoted, no doubt, gave the result of *his experience*, which
was unfavourable to examining the blood, but as he has expressed
it, it does not so appear. His conclusion appears to be founded on
a hypothetical basis, the soundness of which may be called in ques-
tion.

It is an important question for inquiry, whether the blood is, or is
not, the seat of some diseases; whether unhealthy blood has not an
injurious effect in every instance; and if so, what are its modifica-
tions and their effects.

These are legitimate subjects for experimental inquiry. They
can be determined only by experiments carefully and laboriously
made by the scientific physician, whose taste leads him to engage
in such a pursuit, with zeal to support him through it, and opportuni-
ties such as a public hospital affords. Such pursuits belong to the
higher departments of the profession, and must be conducted in the
quiet walks of science. They are, in relation to the ordinary mode

of inspecting and examining the blood by practitioners, like the in-
vestigations of the philosophical navigator, in comparison with the
observations of the trading mariner; the one intent on the natural
history and physical laws of the terraqueous globe, the other limit-
ing his attention to the circumstances which obviously, or are sup-
posed to expedite or impede his voyage, and render secure, or
occasion risk to his vessel. As it is advantageous that the two
characters should be kept distinct in ordinary navigation, so it seems
equally proper in medicine, that the practical part should not be
blended with the scientific, and that the practitioner should keep on
his course guided by certain experience, and so far as science gives
a steady light, enlightened by its rays, leaving the scientific physi-
cian to explain at his leisure, aided by the best methods and instru-
ments of research, the doubtful regions of the profession, and
investigate the mysteries of the system in health and disease. Sir
Charles Scudamore, in the essay referred to, is of a different opinion,
and strongly recommends the attention of the practitioner to the
appearances of the blood, persuaded that they will give "important
information of the power and action of the heart and arteries, and
of the condition of the system itself." And he points out a num-
ber of circumstances, to which, he says, the attention must be
directed in blood-letting; as 1st, "the kind of orifice;" 2dly, the
stream in which the blood flows; 3dly, the state of the pulse during
the bleeding; 4thly, "the apparent density, the colour, and other
appearances of the blood on it first being received in the basin;"
5th, "the time in which it coagulates;" 6thly, "the internal appear-
ance of the coagulated blood;" 7thly, "the texture of the coagulum;"
8thly, "the comparative conditions of the several portions of blood
in different cups;" 9thly, "the appearance and quality of the serum."

These, I admit, and other circumstances, deserve the attention of
the scientific inquirer. I should hope, however, that the mind of
the practitioner and his eye would be more intent on the state of the
patient, than on that of the blood; and having made the orifice,
thought proper in the vein, the blood flowing as he could wish,
that he would concentrate his attention on the effect taking place in
the living system, if the case be an urgent one, and that he would
observe whether there is abatement of pain, or of the sense of op-
pression; whether the pulse rises or falls as the blood flows : and
reflect, judging from the supposed nature of the disease, and the
immediate impression produced on the extent to which the abstrac-
tion of blood should be carried. For my own part, I must candidly
confess, I have never been able to combine in myself the two cha-
racters of the practitioner and inquirer at the same time; and that I
have always found it beyond my power to watch the pulse and
countenance, and feelings of the patient, and study the fleeting ap-
pearances of the blood in a satisfactory manner; and consequently,
when I have attempted the one, even the investigation of a single
property of the blood, I have always been a mere spectator, and have
generally requested the practitioner to judge for himself of the quan-

tity of blood which should be drawn. And, I would recommend the same plan of proceeding to most others; lest, in the ardour and interest of the scientific inquiry, the patient have not that attention paid him which is his due, and humanity suffer in a cause, which should have humanity for its chief object and end; and the name of science in its turn suffer and fall into disgrace amongst those who do not distinguish between the genuine and the false.

III.

AN ACCOUNT OF SOME EXPERIMENTS ON THE BLOOD, CHIEFLY IN CONNECTION WITH THE THEORY OF RESPIRATION.

Connected with the theory of respiration and of animal heat, there are many questions of interest respecting the blood, about which physiologists differ in opinion, and which, consequently, are fit subjects for further inquiry.

Some of the more important and fundamental of these questions, I investigated, experimentally, in the winter of 1837-8; the results were communicated to the Royal Society; they were published in the Philosophical Transactions for the latter year; and were, essentially, as follow, exclusive of the ninth and tenth sections, which have since been added.

1.—*Is blood capable of absorbing oxygen independent of putrefaction.*

To endeavour to satisfy myself on this point, on which, in a former inquiry,[1] I had arrived at a negative conclusion, in opposition to the commonly received opinion, I have employed two methods of experimenting: one, of agitating blood, recently drawn and rapidly cooled, in common air and in oxygen, in a tube of the capacity of two cubic inches, divided into a hundred parts; the other, of agitating it in larger quantities with the same airs, in the very convenient apparatus employed by Dr. Christison when engaged in a similar inquiry, consisting of a double tubulated bottle of the capacity of thirty-two cubic inches, provided with stop-cocks adapted by grinding, to one of which a movable bent glass tube was fitted to connect it with a pneumatic trough, and to the other a perpendicular tube surmounted by a funnel.

The blood subjected to experiment in every instance was prepared by the displacement of its fibrin. This was done by agitating it

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[1] Edinburgh Medical and Surgical Journal, vol. xxxiv. for 1830.

with small pieces of sheet lead in a bottle filled to overflowing and closed with a cork, enveloped in moist bladder and covered over with the same, tied round the neck of the bottle, so as to exclude atmospheric air, whilst in the act of coagulating and of cooling, and to allow, when cooled, of the withdrawal of the cork.

Prepared thus, and rapidly cooled, I have tried different specimens of blood, venous and arterial, of man, of the sheep, of the ox, and dog ; and the results, when the blood has been taken from a healthy animal, have been decisive and consistent. In every instance, whether atmospheric air or oxygen was used, after agitation, there was a marked diminution of the volume of air.

In the examples which it may be advisable to bring forward, I shall confine myself to a few of the experiments I have made on the blood of the sheep.

Using the graduated tube over mercury, sixty-two measures of arterial blood from the carotid artery agitated with thirty-three measures of common air, produced a diminution of two measures ; and sixty-three of the same arterial blood, agitated with thirteen of pure oxygen, a diminution of three ; whilst sixty-three of venous blood from the jugular vein of the same animal, agitated with thirty-three of common air, produced a diminution of six ; and seventy of this blood, with thirteen of oxygen, a diminution of eight.'

In experiments on a larger scale, using the double-mouthed bottle, in which about ten cubic inches of blood were agitated with about twenty-two cubic inches of air, the results were in accordance with the preceding. Thus, when the arterial blood of the sheep was agitated with common air and with oxygen, on turning the stop-cock of the bent tube there was an absorption in one instance of about .3 cubic inch ; and in the other of about .4 cubic inch ; and, with venous blood, in the instance of common air, of about 1. cubic inch, and in that of oxygen, of about 1.35 cubic inch.

These experiments were all made on the blood of the same animal. In experiments on the blood of different individuals of the same species of animal, and on the blood of animals of different species, the results have varied in regard to the degree of absorption, and remarkably so in the instance of the blood of man.

In every instance the absorption or disappearance of a portion of the air has been attended with some change in the colour of the blood ; the venous has invariably acquired the florid vermilion hue of arterial blood, and the arterial has had its florid hue heightened.

The air that has been absorbed, or which disappears, when atmospheric air has been used, in accordance with the commonly

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' Notwithstanding the frothing attending the agitation of blood in air, the absorption or diminution of volume was ascertained with tolerable accuracy by observing the rise of the mercury in the tube. The experiments were made as nearly as possible under ordinary atmospheric pressure, which was easily effected, as the mercurial pneumatic trough used exceeded in depth the length of the tube.

received opinion and the results of Dr. Christison's experiments,[1] has been found to be oxygen.

Relative to the residual air, when pure oxygen has been used, whether on the smaller or larger scale of experiment, no carbonic acid gas has been detected in it in the most carefully conducted trials. When common air has been employed, then a trace of this acid gas has been found in the residual air after agitation with the blood, but not exceeding one per cent. at a temperature between 40° and 50°. I shall notice in detail an experiment with each, as the results are of consequence.

Ten cubic inches of venous blood (its fibrin displaced) from the jugular vein of a sheep, rapidly cooled, were agitated in twenty-two cubic inches of oxygen gas, which had been well washed with lime-water. After an absorption equal to about one cubic inch, some of the gas was expelled and passed into lime-water, the transparency of which it did not in the slightest degree impair.

The same quantity of similar blood was similarly treated with common air, which also had been washed with lime-water. After an absorption equal to about one cubic inch, some of the air was passed into lime-water; it occasioned a just perceptible cloudiness: and eighty measures of it agitated with lime-water were reduced hardly to 79.5.

Taking it for granted that this very minute quantity of carbonic acid derived from the blood existed in the state of gas, and not contained in the aqueous vapour, as is possible, it is matter for consideration from whence it was derived,—whether it was formed at the moment by the action of the air on the blood, or, previously existing in the blood, was now merely expelled. Further on I propose to return to this important question.

When I reflect on the results stated above relative to the absorption or disappearance of oxygen, and compare them with those alluded to formerly obtained, I am not a little surprised at their discrepancy; and I can only account for it by supposing that it may have been owing in part to the difference of season when the two sets of experiments were conducted. The first were made in Malta, in 1829, in the hot months of July and August, when the thermometer in the open air was generally above 80° and occasionally above 90°. The last have been made in England, and principally in January of the year 1838, when the temperature of atmosphere was occasionally low, the greater part of the time below the freezing point, and often so low as 20°. From what I have witnessed, I am induced to infer, that the higher the atmospheric temperature is, and the less necessity there is for the production of animal heat, the less difference there is between venous and arterial blood, and the less power the former has of combining with oxygen, and of forming or evolving carbonic acid. In Malta I carefully compared the blood of the jugular vein and carotid artery of a sheep during the season mentioned; when

coagulated and still hot, there was no perceptible difference in their colour; in each it was less florid than the arterial blood of the same animal in England in winter, and less dark than the venous; its hue was as it were a mixture of the two. And in this observation I could not be mistaken; the circumstances under which it was made precluded mistake; vessels of the same size were used; similar quantities of blood were introduced, and they were seen in the same light side by side. Referring to Dr. Crawford's Treatise on Animal Heat, I find in confirmation that he had arrived at the same conclusion more than fifty years ago, from his own experiments on the influence of heat and cold on the colour of the blood, and the quantity of oxygen consumed in respiration. In the most decisive experiments detailed by him, he states, that " the venous blood (of a dog that had been immersed in warm water for half an hour) assumed very nearly the hue of the arterial, and resembled it so much in appearance that it was difficult to distinguish between them."[1] And, in another experiment, the reverse of this, in which a dog was immersed in cold water at 45°, for about a quarter of an hour, he says, that the blood now taken from the jugular vein appeared to him, as well as to several other gentlemen who assisted, " to be the darkest venous blood we had ever seen."[2] In my communication to the Royal Society, in corroboration of the view taken, I laid too much stress on an inferred higher temperature in winter compared with summer of the deeply-seated parts of animals, specifying that of the heart and rectum of sheep, and which has not been fully borne out by my later observations; those indicating that the average heat in summer is not less, but is very little higher than in winter. But, as regards the theoretical conclusion, so small a difference of animal temperature, contrasted with the very great difference of atmospheric temperature—the former not exceeding 1°, the latter often 50°—can be considered very little less cogent, than if, as I erroneously supposed, the winter temperature of the heart were actually the highest.

2.— *Does the blood, especially venous blood, contain carbonic acid capable of being expelled by agitation with another gas, as hydrogen or oxygen.*

In a paper published in the Philosophical Transactions for 1835,

[1] Experiments and observations on Animal Heat, p. 309. Second edition.
[2] Perhaps the influence of atmospheric temperature may in some measure help to reconcile the apparently discordant results of numerous observations on the colour of arterial and venous blood ; thus on one side Haller says, " Sæpe comparavi arteriæ pulmonalis cavæque venæ sanguinem, cum arterioso sanguine venæ de pulmone reducis, nunquam in multis experimentis, aliquam in colore utriusque sanguinis diversitatem vidi, neque Harveius vidit, neque Guilielmus Cheselden, neque cl. Knight, neque cl. noster auditor Evertsen, neque ipse contrariæ opinionis auctor Hambergerus."—Element. Physiol. ii. p. 11.

Dr. Stevens has answered this question in the affirmative; he main-
tains that carbonic acid gas exists in venous blood; that it may be
expelled by oxygen or by hydrogen, although not by the air-pump;
and he supposes that the difference of effect is owing to a peculiar
attraction for carbonic acid exercised by these gases.

To endeavour to resolve my doubts on this important point, I
have had recourse to the apparatus already mentioned, viz. the
graduated tube with the mercurial pneumatic trough, and the
double-mouthed bottle furnished with stop-cocks, &c. as being well
adapted for simple and decisive experiments.

By means of the graduated tube, I have agitated venous blood
in hydrogen over mercury, as about a cubic inch of each, and other
proportions, and have left the blood exposed to the influence of the
gas for several hours; and I have made similar trials with it, using
larger quantities in the double-mouthed bottle, as sixteen cubic
inches of each, and also other proportions.   The results have been
either of the same negative character, or, if different, indicating
only the disengagement of carbonic acid gas in an extremely
minute quantity.   In all the experiments with the graduated tube
in which fresh blood was used, whether of man or of the sheep,
the fibrin displaced out of the contact of the air, on agitation with
hydrogen, there was no sensible increase of the volume of the gas,
and no diminution of it when it was transferred to, and shaken
with, lime-water.   And in the best experiments, on a larger scale,
with the double-mouthed bottle, when most attention was paid to
all the circumstances likely to insure accuracy, (as in the first
instance the exclusion of air from the blood, and in the second the
having it of the temperature of the bottle and of the room,) the
results have been similar, and negative.   I shall describe a small
number of experiments, those, the results of which appeared least
ambiguous.

Seven cubic inches of sheep's blood from the jugular vein, its
fibrin broken up by agitation with lead whilst coagulating, out of
contact with air, and cooled under water, were agitated with hydro-
gen, (twenty-five cubic inches,) previously well-washed with lime-
water, and which, tested by lime-water, after this precaution were
found perfectly free from carbonic acid.   On turning the stop-cock
of the bent tube connected with distilled water, no change of
volume was indicated, and the blood was agitated again with the
same result.   By means of the perpendicular tube distilled water
was admitted, and some of the gas expelled; first a cubic inch into
a graduated tube filled with lime-water, and next about four cubic
inches into a vial filled with distilled water, and in which after-
wards a little lime-water was added to the gas; in neither could
any traces of carbonic acid be detected; the lime-water remained
transparent.

Ten cubic inches of venous blood, taken by a large orifice from
the arm of a young man threatened with hæmoptysis, the fibrin
broken up in the same manner as the last, and rapidly cooled under

water in a running stream to the temperature of the room, 51°, were similarly treated with hydrogen, and with precisely the same result; after having been twice well shaken, on turning the stop-cock, there was no change of volume. The blood was kept in contact with the hydrogen over night, the stop-cocks closed. The night, that of the 23d of January, was severely cold; at 11 o'clock the following morning the temperature of the room was only 45°; now on turning the stop-cock of the bent tube, the water rose in it to the extent of about one eighth of a cubic inch.

Twenty-four cubic inches of the mixed arterial and venous blood of the sheep, collected and prepared with similar precautions, were divided into two portions of about twelve cubic inches; one was agitated in the double mouthed bottle with hydrogen after the introduction of a little milk of lime, the other without this addition. The result in each instance was the same; on opening the stop-cock, after the agitation, the water rose a very little in the bent tube,—about one-twentieth of a cubic inch.

The results of some of the trials already described on the action of oxygen on venous blood, both pure and mixed with azote, in the form of common air, are very consistent with those just detailed on hydrogen. Previously to stating some other results in quest of fresh evidence on the same subject, it may be advisable to notice particularly the power which blood possesses of absorbing carbonic acid.

From experiments which I made on blood and serum in 1824 and 1828,[1] I inferred that each is capable of absorbing about an equal volume of this acid gas. I now find that when *pure* carbonic acid gas is brought in contact with blood or serum over mercury, and moderately agitated under ordinary atmospheric pressure, the absorption of gas exceeds the volume of the fluid, both in the instance of blood and serum. The results of some experiments are exhibited in the following table. The majority of them were obtained between a temperature of 40° and 45°; the three last at about 51°. When the venous and arterial blood, and the serum tried, were from the same animal, the numbers expressing the results are entered in the same line.

[1] Philosophical Transactions for 1824. Edinburgh Medical and Surgical Journal, vol. xxx.

Volume of carbonic acid gas absorbed by 100 *parts of blood and serum.*

No.	Animal.		Venous blood.	Venous serum.	Arterial blood.	Arterial serum.
1	Sheep	-	160	—	167	
2	Sheep	-	155			
3	Sheep	-	—	—	133	.
4	Sheep	-	142			
5	Sheep	-	—	—	150	
6	Sheep	-	166			
7	Sheep	-	—	—	—	120
8	Ox	-	194			
9	Ox	-	181			
10	Man	-	118			
11	Heifer	-	120	—	—	117
12	Sheep	-	14S	125	141	125
13	Sheep	-	—	118	—	118
14	Man	-	153			
15	Sheep	-	160			
16	Sheep	-	—	—	159	

The effect of the absorption of the gas to perfect saturation was on the arterial and venous blood the same; it rendered both very dark; the serum it rendered more liquid, which was well marked by diminished tendency to froth on agitation.

I shall now proceed to notice the trials which I have instituted of agitating blood and serum, to which a known quantity of carbonic acid had been added, with one or more of the gases considered by Dr. Stevens as exerting an attraction on carbonic acid, and by that means expelling it.

From the experiments which I have made on serum, it appears in its healthy state incapable of absorbing oxygen, or of immediately furnishing carbon to form carbonic acid; and in no instance in which I have agitated it with common air or with hydrogen, when obtained from the blood of a healthy animal, has there been any indication of the disengagement of gas; it therefore is peculiarly well fitted for the trial in question.

To nine cubic inches of serum from the mixed blood of the sheep one cubic inch more of serum was added, containing a cubic inch of carbonic acid, with which it had been impregnated over mercury. The mixture of the two was introduced into the double-mouthed bottle without delay, and well agitated with twenty-two cubic inches of common air. On turning the stop-cock there was no change of volume. The serum was transferred, and there was added to it, with as little motion as possible, another cubic inch of serum, containing the same quantity of carbonic acid. Now poured back into the bottle and agitated, on opening the stop-cock a little air was disengaged; it was collected and found equal to $\frac{4}{100}$ths cubic inch. The serum was left exposed to the action of the air in

the bottle over night, the stop-cocks closed; the following morning
on opening the stop-cock of the bent tube no air was expelled; on
the contrary, there was a just perceptible rise of water in it.   The
experiment was carried further: the serum was transferred to a
vial and closed, and the double-mouthed bottle was filled with
hydrogen.   The serum was returned and well agitated with the
hydrogen.   On turning the stop-cock $\frac{14}{100}$ths of a cubic inch of air
was expelled.   It was agitated a second time without further
expulsion of air, and left in contact with hydrogen for more than
twelve hours without any further effect.   Thus it appears that of
the two cubic inches of carbonic acid gas introduced into the serum,
only one fifth of a cubic inch was expelled by successive agitation
with atmospheric air and with hydrogen.

One cubic inch of venous blood of a man which had absorbed
1.2 cubic inch of carbonic acid, was mixed with twelve cubic
inches of similar blood, and agitated with hydrogen in the double-
mouthed bottle.   A very little air only was expelled, viz. $\frac{4}{100}$ths
cubic inch.

To fifty-five measures (1.1 cubic inch) of venous blood of a sheep,
twenty measures of gas were added over mercury, composed of
about equal parts of oxygen and carbonic acid.   After agitation
about seventeen measures were absorbed, and the blood had
acquired a florid hue; ten measures more of oxygen were added;
there was no further absorption.   The tube was transferred to
lime-water and agitated; the residual air was not diminished; it
amounted to thirteen or fourteen measures (the froth prevented
precision in marking the quantity,) which possessed the properties
of oxygen, as tested by the taper.

To twenty-eight measures of venous blood of a sheep, which had
absorbed twenty-six of carbonic acid, forty-nine of oxygen were
added in the graduated tube over mercury: after agitation the blood
had acquired the florid arterial hue, and there appeared to be an
expansion of one measure.   Transferred to lime-water there was
an absorption of one or two measures, and no more.

I shall notice one experiment more, in which blood nearly satu-
rated with carbonic acid was exposed to oxygen, a membrane
intervening.   Forty-seven measures of venous blood, which had
absorbed thirty-three of carbonic acid, were introduced into a glass
tube half an inch in diameter, closed at one end with gold-beaters'
skin, and when filled with blood, at the other end also; it was
placed over mercury in a small receiver, and thirty-seven measures
of oxygen were added to it, under a diminished pressure of about
one inch.   After twenty-four hours there was no change of volume;
the blood in the tube had acquired throughout the arterial hue; the
gas, thirty-eight measures, transferred to lime-water and agitated,
diminished to thirty-two.

The tube was now placed under a receiver, and the air exhausted
by the air-pump; a good deal of air was disengaged in the form of
froth, and the gold-beaters' skin was so distended that it appeared

ready to burst; after three or four minutes air was re-admitted; a notable portion of gas was found free between the membrane and blood; thus showing, that in oxygen gas carbonic acid gas is less freely exhaled through a membrane than *in vacuo.*

The results of this second set of experiments are in accordance with those of the first. The inference I am induced to draw from both is rather of a negative kind, and unfavourable to the conclusion of Dr. Stevens, already referred to, at least in a strict and general sense. I think it right to express myself thus reservedly, reflecting on some of the experiments in which a very little carbonic acid gas appeared to be extricated on agitating blood with hydrogen; and believing that Dr. Stevens, and other able inquirers, could not have been misled on a point so little exposed to fallacy.

Relative to the effects which Dr. Stevens refers to the attraction of one gas for another, they appear to me, from what I have witnessed in carrying on the inquiry, to admit of explanation on Dr. Dalton's theory of mixed gases, and that in no instance is the effect of disengagement of air from a fluid, agitated with another kind of air, greater than were it agitated in a vacuum.

3.— *What is the condition of the alkali in the blood in relation to carbonic acid?*

On this point much difference of opinion exists amongst inquirers; some believing that the alkali, or at least a portion of it, is uncombined, or combined merely with water, or with water and albumen; some that it is united to carbonic acid in the state of carbonate; others that it is saturated with this gas, and in the state of bicarbonate.

The subject, it must be confessed, is one of great difficulty, and very perplexing; partly from the nature of the blood, liable to great variations during life, and to rapid change after death; and partly also from the nature of the alkaline carbonates, hardly less disposed to change than the blood itself, from variation of circumstances, and to pass from one degree of combination into another.

The bicarbonate of soda, I believe, like the bicarbonate of ammonia, can only exist in perfection in the solid state. In dissolving, I find, when exposed to the atmosphere, it gives off a part of its acid, and still more when it is agitated with common air, and more still with hydrogen, and in a greater degree the higher the temperature. This is not favourable to the idea that it exists in the blood, especially when it is considered that this fluid may be exposed to a temperature of 212° without disengaging carbonic acid, of which I have had proof in several trials.

Sulphuretted hydrogen does not expel carbonic acid from the alkalies in solution in water. Bicarbonate of soda dissolved to saturation in distilled water absorbs, I find, 143 per cent. of its volume of this gas; whilst the serum of blood (it was sheep's that

was tried) dissolved 207 per cent., arterial blood 235, and venous 290. This, too, is unfavourable to the same idea; as is also the large proportion of carbonic acid which blood, it has been shown, is capable of absorbing.

Supertartrate of potash occasions an effervescence, when mixed in substance with a solution of the sesquicarbonate, but not of the carbonate of soda; and the effect is similar, whether the mixture be made over mercury, air excluded, or in an open vessel exposed to the atmosphere. The supertartrate of potash also, I find, mixed with blood and agitated with common air, acts as with the sesqui-carbonate, and occasions a disengagement of air, and both from arterial and venous blood, and from serum; and the air I have ascertained is carbonic acid.

From these facts, may it not be inferred that the alkali in the blood, in its normal or healthiest condition, is neither in the state of carbonate nor of bicarbonate, but of sesquicarbonate? The power of the blood to absorb carbonic acid and sulphuretted hydrogen accords best with this view, and some other important properties of the fluid are, I believe, in harmony with it.

The sesquicarbonate, I may add, seems to be the state of rest of the alkali in combination with carbonic acid, under ordinary cir-cumstances of exposure to the atmosphere. Thus the native compound is the sesquicarbonate, as is also, I believe, the effloresced salt.[1] And I find that although a solution of the bicarbonate may be brought by the air-pump to the state of sesquicarbonate, it cannot be reduced to that of the carbonate; after it has ceased to give off any air *in vacuo*, it effervesces with the supertartrate of potash; and if evaporated to dryness over sulphuric acid under an ex-hausted receiver, on being subjected to heat, it disengages carbonic acid gas.

4.—*Does the blood contain any gas capable of being extricated by the air-pump?*

On this subject also there has been much difference of opinion. Our distinguished countryman Mayow, more than a century ago, stated that blood, especially arterial blood, effervesces *in vacuo*, which he attributed to the disengagement of air, his spiritus nitro-aërius.[2] Sir Everard Home, on the authority of Mr. Brande, in

[1] On exposing carbonate of soda in excess to carbonic acid gas over mer-cury, the gas is rapidly absorbed with the expulsion of part of the water of crystallization, so as to produce an appearance of deliquescence, and the sesquicarbonate is formed.

[2] Johannis Mayow, Opera omnia. Hagæ Com. 1681, p. 133. The follow-ing is the passage referred to—" Si sanguis in vase aliquandiu servatus, in vitrum colliocetur, ex quo aer per antliam aeream exhauritur, sanguis iste in superficie, quâ idem colorem floridum obtinuit, leniter effervescet, et in bullas assurget. Sin autem sanguis arteriosus adhuc incalescens, in loco aere vacuo positus fuerit, idem mirum in modum expandetur, et in bullulas penè

1818, asserted that blood, both venous and arterial, under the exhausted receiver evolves a large quantity of carbonic acid gas, an ounce of blood as much as two cubic inches of gas.[1] I repeated the experiment shortly after, but without confirming the result; neither by the air-pump, nor by heat applied even to coagulation of blood and serum in close vessels, did I succeed in demonstrating the extrication of this acid.[2] Since that time the experiment of the air-pump on the blood has been frequently made, and by observers of great accuracy, as by Drs. Duncan and Christison in this country, by MM. Tiedemann, Gmelin, Mitschenlich and Müller on the continent, and recently by MM. Bischoff and Magnus. With the exception of the last mentioned inquirers, the results have been negative. MM. Bischoff and Magnus, on the contrary, state that by careful exhaustion they have obtained gas from the blood; the former a small quantity of carbonic acid gas from venous blood, and a very little gas of a different kind from arterial, the nature of which he did not determine;[3] the latter a notable quantity, and from arterial as well as venous blood, and not only of carbonic acid, but also of oxygen and azote.[4]

M. Magnus attributes the failures of former experimenters to their having used pumps of imperfect exhausting power, or to their not having carried the exhaustion sufficiently far. In my early and first trial I employed the air-pump belonging to the laboratory of the Royal Institution, which was an excellent one, and which I then believed was in good order; but I might have been mistaken.

To endeavour to satisfy myself on this point, I have had an air-pump constructed for the purpose, under the direction of an able artist, Mr. Ross, of 33 Regent street, already distinguished for his excellent achromatic microscopes, and it has answered perfectly. When the exhaustion has been carried as far as possible, the difference of level of the mercury in the siphon-gauge has not exceeded a tenth of an inch, and over water has not exceeded the quarter of an inch.

Experimenting with this machine on blood, collected with such precautions as I believe to have been adequate to insure accuracy of results, in a majority of instances the disengagement of gas has been rendered manifest, and both from arterial and venous blood.

I shall briefly mention the trials which I consider most con-

infinitas elevabilur: id quod à particulis ejus exæstuantibus, inque motum posilis. partim ab aere particulis ejus interspersis oriri verisimile est."

[1] Philosophical Transactions for 1818, p. 181.

[2] Ibid. 1823, p. 516.

[3] Commentatio de novis quibusdam experimentis chemico-physioligicis ad illustrandam doctrinam de respiratione institutis Heidelb. 1837—p. 9 et seq.

[4] Annales des Sciences Nat. tom. viii. p. 79, et seq. contain a translation of M. Magnus's paper, with a figure of the apparatus, &c. used; the original appeared in Poggendorf's Journal, vol. xl. part 3.

clusive, and which satisfied me in spite of an opposite pre-existing bias.

Vials provided with well-ground glass stoppers were filled with distilled water, deprived as much as possible of air by the air-pump; they were then placed under the receiver and kept in vacuo until all adhering air was removed; the stoppers were now introduced, all air being excluded, and they were instantly immersed in distilled water, which had been well boiled. Thus prepared they were taken to an adjoining slaughter-house, where they were filled with sheep's blood in the following manner, without its coming in contact with the air. For venous blood the jugular vein was exposed; two ligatures were applied to it; the vein was divided between the two, and the upper part, slightly detached, was introduced into a prepared vial under water the instant the stopper was withdrawn, and laid open. The heavier blood proceeding from the vessel of course expelled the water; and when it was supposed to be all expelled, the stopper was restored, and the vial was replaced in the water. In the instance of arterial blood it was collected in the same manner from the carotid artery. In some trials the blood was allowed to coagulate undisturbed; in others the fibrin was detached, and the liquidity of the blood preserved by agitating it, the instant the stopper was replaced, with some mercury, introduced with the distilled water, and equally deprived of adhering air.

In about half an hour from the abstraction of the blood, in every instance, it was subjected to the air-pump. The instant the stopper was withdrawn, the vial was placed in a small receiver on the plate of the pump, and covered with a little larger receiver, and the air as soon as possible exhausted. No appearance of disengagement of gas was perceptible until the exhaustion was nearly complete; then it was sudden, sometimes considerable, even to overflowing, in the form of bubbles, and it continued some time. The results were not distinctly different, that I could perceive, whether venous or arterial blood was used; I am disposed to think, on the whole, that less air was disengaged from the arterial blood than from the venous.

When blood allowed to coagulate in the vessels was tried, the results varied a little, and appeared to me instructive. At first, on exhaustion, only a few particles of air were disengaged; no more, it might be supposed, than were derived from the contact of the end of the stopper. In two experiments, such was the appearance for at least five minutes, conveying the idea that no air was extricated; then abruptly a bubble or film burst with some force, as was denoted by the scattering of the particles of blood; and a bubbling commenced and continued, rendering the indications of extrication of gas unquestionable. And in conformity with this result, I may remark, I have never succeeded in obtaining indications of air in the blood in operating on it by the air-pump, if confined in a detached portion of vein, or in the heart of animals, the great vessels,

previous to excision, having been tightly secured by ligature. In no instance of this kind have I witnessed any distention, such as occurs if air be admitted previous to the application of the ligatures: clearly indicative, it appears to me, that a very slight compressing force is sufficient to confine the air in the blood; or rather, I should say, prevent its substance assuming the elastic state; and further, the probability, that the quantity of air so condensed is small.

I have stated that in the majority of instances the indications of the disengagement of air from blood *in vacuo* have been manifest. Exceptions, however, have occurred, and those clear and decisive; inducing me to believe that the quantity of air condensed in the blood is variable; and that there are times when it is in excess, and times when in deficiency, and when totally wanting, connected with regularly changing states of the functional system. I hope at a future time to be able to recur to this part of the subject, on which at present I have collected but a few facts. I may add, that such facts tend in part to reconcile some of the discrepancies referred to in the beginning of this section.

5.— Of the air or gases contained in the blood capable of being extricated by the air-pump.

As already mentioned, M. Magnus has stated that these gases are carbonic acid, oxygen and azote in notable quantities. Taking the mean of ten of his experiments on the blood of the horse and calf (five on arterial and five on venous) the total quantity of the mixed gases he obtained was, in the instance of arterial blood, 10·4 per cent. per volume, and in that of venous 7·6 per cent.; the former consisting of about

> 6·5 carbonic acid,
> 2·4 oxygen,
> 1·5 azote;

the latter of

> 5·5 carbonic acid,
> 1·1 oxygen,
> 1·0 azote.

On a subject of so much importance, it is very desirable that the experiments of M. Magnus should be repeated and verified; for until this is done, considering the physiological history of the blood, it will be difficult to avoid doubt, it will be difficult to depend on them with that degree of confidence which is justly due only to well-authenticated facts.

The ingenious apparatus employed by M. Magnus being of complex construction, and not easily used excepting in a well-appointed laboratory, I must leave the repetition of his highly interesting experiments to those inquirers who are more happily situated than myself for engaging in them. I shall limit myself to the detail of

some experiments instituted with a view of testing M. Magnus's general results.

As a solution of potassa has the property of absorbing carbonic acid gas, it follows that if mixed with the blood previous to being subjected to the air-pump, it will prove in some measure a test of the kind of air which the blood is capable of affording.

With this intent two vials were prepared, the same as before used, one filled with distilled water, the other with a weak aqueous solution of caustic potash, both carefully deprived of air by the air-pump. Observing the same precautions as before, a portion of venous blood from a sheep was received into each of them. When less than half of the water and of the solution, as well as could be guessed, was expelled, the vials were closed with the glass stoppers belonging to them, and instantly immersed in water, and as soon as possible subjected to the air-pump. The results of exhaustion in the two instances were perfectly distinct. From the blood mixed with water gas was disengaged; there was a continued ascension of bubbles. From the blood mixed with the alkaline solution no gas was liberated, excepting a bubble or two, which might fairly be considered as entangled air derived from contact of the blood with the stopper.

A similar comparative trial was instituted with arterial blood of the sheep, and the results also were similar and equally well marked.

Considered as test experiments, the first inference from these results is, that carbonic acid gas, or an air absorbable by a solution of potash, is disengaged both by venous and arterial blood in vacuo; and next, that no other gas is disengaged from either of them, neither oxygen nor azote, each of which is unabsorbable by the solution in question. I have repeated the experiment twice on the venous blood of man, and twice on the venous blood of the sheep, and twice on the arterial blood of the latter, without variation of results; and they are more to be depended on, as the alkali has the effect of preserving the blood liquid.

I may mention another method which I have employed as a test experiment; and, first, in relation to carbonic acid. If the blood contain a notable portion of this gas, capable of being extricated by the air-pump, it necessarily follows, that, when subjected to the action of the air-pump, and deprived as far as possible of its fixed air by this means, it will be capable of absorbing a larger quantity of carbonic acid than previous to the exhaustion. The following table contains the results of three comparative trials on the venous blood of the sheep, its fibrin separated or detached in the usual manner.

Table showing the absorption of carbonic acid gas by

Venous blood subjected to the air-pump.		
Volume of blood used.	Volume of carbonic acid introduced.	Volume of gas absorbed.
27	46	38
27	45	39
27	46	35
Venous blood not subjected to the air-pump.		
Volume of blood used.	Volume of gas introduced.	Volume of gas absorbed.
27	47	37
27	45	39
27	45	34

Although I have thought it right to notice these results, and although they are in accordance with the preceding, I do not attach value to them, excepting as tending to show that the quantity of carbonic acid gas extracted by the air-pump, when the blood affords it in vacuo, is small.

The same mode of reasoning suggested comparative trials of the absorbent power of arterial blood for oxygen, before and after exhaustion by the air-pump, as a further test experiment, whether arterial blood contain oxygen in a free state, that is, admitting of being extricated by the removal of atmospheric pressure. The result of this trial also has been negative; its power of absorbing oxygen has not appeared to be at all increased by exhaustion; this at least was the result of one experiment carefully conducted.

6.--*Is any oxygen contained in the blood not capable of being extricated by the air-pump?*

Before I was acquainted with the researches of M. Magnus, I had instituted some experiments to endeavour to determine whether any oxygen in a free state, or in a condition approximating to that state, exists in the blood, and especially in the arterial, admitting of being detected by means of substances possessing a strong attraction for oxygen, or of being expelled by substances of greater solubility in blood than oxygen. Hydrogen, phosphuretted-hydrogen, sulphuretted hydrogen, nitrous oxide and nitrous gas, it appeared

advisable to try, as belonging to the first class of substances, and carbonic acid gas as belonging to the latter.

The results with hydrogen, sulphuretted hydrogen, nitrous oxide and carbonic acid gas, were of a negative kind. Neither using arterial blood, nor blood which had been agitated with oxygen, and which had absorbed or made to disappear a certain quantity of this gas, could I detect any indications of its presence, either by combination or expulsion. In the instances of nitrous oxide and carbonic acid, however, it may be worthy of remark, that the blood which had been agitated with oxygen absorbed less of either of these gases than it did before it was so treated.[1]

The results with phosphuretted hydrogen, the spontaneously inflammable species, were of an ambiguous kind, not sufficiently clear to deduce from them any satisfactory conclusion. In one trial, serum of the venous blood of the sheep absorbed nine per cent. of this gas; venous blood 11·3 per cent.; and arterial 5·8 per cent.[2]

The results with nitrous gas were of a different kind, and may be deserving of being specially noticed.

The blood used was that of the sheep, prepared in the usual manner. The experiments were made during the very cold weather which prevailed in the beginning of the year already mentioned, and the difference of colour between the venous and arterial blood was very strongly marked.

(1).— On arterial blood.

1. Fifty-three measures of this blood were agitated over mercury, with forty-six measures of nitrous gas; there was a diminution of volume of seventeen measures.

2. Fifty-three measures of the same blood (another portion) were agitated with nine of oxygen; two measures were absorbed.

3. Fifty measures of this blood, so treated with oxygen, were agitated with forty-seven of nitrous gas; there was a diminution of twenty-two measures.

(2).— On venous blood.

1. Fifty-three measures of this blood were agitated with fifty of nitrous gas; there was a diminution of ten measures.

[1] Nitrous oxide, I find, is absorbed in about the same proportion by venous and arterial blood, and by the serum of blood, and also in about the same proportion by water. Thus, at the temperature of 45° over mercury, using the blood and serum of the same animal (the sheep) thirty-two measures of each absorbed twenty-two measures of this gas, and thirty-two of distilled water absorbed 21·5.

[2] Supposing the gas decomposed—the phosphorus uniting with the oxygen in the blood, the apparent smaller absorption by arterial than by venous blood is what might be expected, on the idea that the former kind of blood contains most oxygen.

2. Fifty-three measures (another portion) were agitated with ten of oxygen; five measures were absorbed.

3. Fifty-one measures of blood so treated were agitated with forty-nine of nitrous gas; there was a diminution of seventeen measures.

The residual air in each instance was examined, and was found to be a mixture of nitrous gas and azote with carbonic acid gas. The azote, it may be presumed, was introduced with the nitrous gas; it was in the same proportion as that which adulterated it, viz. about four per cent. That the residual air was free from carbonic acid was inferred from the circumstance, that in comparative experiments with and without addition of a portion of solution of caustic alkali, there was no difference in the proportion of nitrous gas absorbed: and it was corroborated by another circumstance, viz. that after the absorption of the nitrous gas, the blood was capable of absorbing seventy per cent. of carbonic acid gas;[1] and farther by the result, that when nitrous gas is added not to saturation, the whole of it is absorbed.[2]

As regards the blood itself, the colour of both venous and arterial was altered; both were rendered darker and browner, as if a minute quantity of nitric acid had been added to them, a change long known to be occasioned by nitrous gas. In the degree of change there was, however, a difference; in the instance of arterial and of oxygenated arterial blood, it was more strongly marked than in that of the venous.

In conjunction with the unsuccessful attempts with the other gases already mentioned, do not the results just described indicate that a portion of oxygen exists in the blood, not capable of being extracted by the air-pump, and yet capable of entering into combination with nitrous gas, and which exists in largest proportion in arterial blood? Unless this conclusion is adopted, it must be supposed either that the nitrous gas which disappears is decomposed, or that it combines directly with the red particles; neither of which suppositions is well supported by facts. The circumstance that no azote is disengaged, is not favourable to the idea that the nitrous gas is decomposed; and the difference of effect in the instances of venous and arterial blood, and, after and before agitation with oxygen, is not in accordance with the notion of direct combination. I

[1] To some venous blood of a sheep, which absorbed 182 per cent. of carbonic acid gas, so much of a solution of pure hydrate of potash was added, that it absorbed 218 per cent. of the acid gas; seventeen measures of this blood with excess of alkali, agitated with fifty-one of nitrous gas, absorbed 5·5 measures; sixteen of the blood without the excess of alkali absorbed five measures: thirty-four measures of carbonic acid gas were added to the latter, the excess of nitrous gas being left in the tube; on agitation, twelve measures of the carbonic acid were absorbed.

[2] In one experiment eighty-four measures of the venous blood of the sheep were agitated with ten of nitrous gas over mercury; the whole of the gas was absorbed: twelve of oxygen were added; on agitation again there was no further absorption.

may mention another fact which seems to have the same bearing: serum, which does not absorb oxygen, I find also does not absorb nitrous gas, excepting in about the proportion in which water rbsorbs it: and, further, in corroboration, I may mention, that as blood putrefies, whether arterial or venous, its power of absorbing nitrous gas diminishes; and that it is also diminished by being agitated with phosphuretted hydrogen. Thus the arterial blood of a sheep, which before agitation with phosphuretted hydrogen absorbed 45·3 per cent. of nitrous gas, after agitation with it absorbed 7·4 per cent. less; and, after it had become putrid, it absorbed 20 per cent. less. According to my observations, arterial blood does not lose its peculiar florid hue under the action of the air pump. Is not this also in favour of the above inference, that a portion of oxygen is retained by the blood resisting extraction by the air-pump? I find also, that when venous blood is agitated with oxygen, and subjected to the air-pump, it, in like manner, retains its acquired florid vermilion hue, and likewise a power of absorbing an additional quantity of nitrous gas.

7.— *When oxygen is absorbed by the blood, is there any production of heat?*

To endeavour to determine this point, of so much interest in connection with the theory of animal heat, a very thin vial, of the capacity of eight liquid ounces, was selected and carefully enveloped in bad conducting substances, viz. several folds of flannel, of fine oiled paper, and of oiled cloth. Thus prepared, and a perforated cork being provided, holding a delicate thermometer, two cubic inches of mercury were introduced, and immediately after it was filled with venous blood, kept liquid as before described. The vial was now corked and shaken; the thermometer included was stationary at 45°. After five minutes that it was so stationary, the thermometer was withdrawn, the vial closed by another cork was transferred inverted to a mercurial bath, and 1½ cubic inch of oxygen was introduced. The common cork was returned, and the vial was well agitated for about a minute; the thermometer was now introduced; it rose immediately to 46°, and continuing the agitation, it rose further to 46·5, very nearly to 47°. This experiment was made on the 12th of February, 1838, on the blood of the sheep.

On the following day a similar experiment was made on the venous blood of man. The vial was filled with eleven cubic inches of this blood, its fibrin broken up in the usual manner, and with three cubic inches of mercury; the temperature of the blood and mercury was 42·5, and the temperature was the same after the introduction of three cubic inches of oxygen. The temperature of the room being 47°, a fire having shortly before been lit, the vial was taken to an adjoining passage where the temperature of the air was 39°. Here the vial was well agitated, held in the hand with

thick gloves on as an additional protection. After about three quarters of a minute the thermometer in the vial had risen a degree, viz. to 43·5.

On the 14th of the same month a third experiment was made on venous blood from the jugular vein of a sheep. The vial was filled with 3·5 cubic inches of mercury and eleven cubic inches of blood. The thermometer in the bottle, left five minutes, was stationary at 49°; the temperature of the mercurial bath was 49°; the air of the room was 52°; a thermometer with its bulb moistened was 45°, which I mention because the outer covering of the vial was moistened with some blood which had overflowed. After three cubic inches and a half of oxygen had been introduced, before agitation, the thermometer was still 49°. The bottle was briskly shaken for about half a minute; now, on observing the thermometer, it was found at 50°; the vial was again agitated; there was no further increase of temperature: after ten minutes it had fallen to 49°.

I shall relate one experiment more, and that on arterial blood. It was made on the 14th of February, and in the same manner as those on the venous blood. Before and after the introduction of the oxygen, the blood, which was from the carotid artery of the sheep, was 45°; after agitation with oxygen it rose to 45·5: this was done when the temperature of the air was 39°.[1]

In a former part of this paper I proposed to recur to the question, Is the fixation of oxygen in the blood attended with the formation of carbonic acid gas? The change of colour accompanying the fixation of oxygen by the blood, so different from that produced by carbonic acid, and the effect of nitrous gas before and after, seem to be most in favour of the idea, that the oxygen, in the first instance, is simply absorbed, and that the heat evolved is merely the effect of its condensation; or, that if any of it enters into immediate union with the carbon, it is only a small part of the whole.

8.—*Theoretical remarks.*

Should the results detailed in the preceding pages be confirmed on repetition, they can hardly fail to have some effect on the theory of respiration and animal heat.

[1] Sir Charles Scudamore, in his Essay on the Blood, at page 59, states that venous blood cools much more slowly in oxygen gas than in atmospheric air; that the same blood divided into two cupping glasses, "after an interval of eight minutes from the beginning of the experiment," exhibited a difference of eight degrees—that exposed to oxygen being 85°, that to atmospherical air 77°. As in this instance, the surface of the blood only was exposed, and there was no agitation—the effect described could not have been from the absorption of oxygen;—an appreciable portion of oxygen could not have been absorbed in so short a time. How it was accomplished (the retarded cooling) I shall not attempt to explain; I apprehend there must have been some mistake, or fallacy, either in observing or recording the result.

As regards the former, they appear to me to tend to show that the lungs are absorbing and secreting, and perhaps exhaling organs, and that their peculiar function is to introduce oxygen into the blood, and separate carbonic acid from the blood.

As regards animal heat, they appear to favour the idea, that it is owing, first, to the fixation or condensation of oxygen in the blood in the lungs in conversion from venous to arterial; and secondly, to the combinations into which it enters in the circulation in connection with the different secretions and changes essential to animal life.

In illustration of what I imagine the secreting power of the lungs, I may mention the difference of effects in an instance of death by strangulation, and another by exhaustion of air from the lungs by the air-pump. A full grown guinea pig was the subject of experiment in each. The one killed by strangulation died in about a minute after a cord had been drawn tightly round its neck; the other, placed on the plate of the air-pump, and confined by a receiver just large enough to hold it, lived about five minutes after the exhaustion had been commenced, the pump the whole time having been worked rapidly. The bodies were immediately examined. The heart of the strangled animal was motionless; it was distended with dark blood; twelve measures of the blood, broken up and agitated with twenty-nine of carbonic acid gas, absorbed eighteen measures, or 150 per cent. The heart of the other guinea pig was also distended with blood, but of a less dark hue. Its auricles were feebly acting; the lungs were paler than in the former, and more collapsed: ten measures of blood from the heart, broken up and agitated with fifty of carbonic acid gas, absorbed thirty-seven measures, or 370 per cent.!

Further, in illustration of this supposed secreting power of the lungs, I might adduce the condition of the blood in disease, and in instances in which I have examined it after death from disease, in the majority of which I have found the blood loaded with carbonic acid, as indicated both by the disengagement of this gas when the blood was agitated with another gas, and by the comparatively small proportion of carbonic acid which the blood was capable of absorbing. This condition of the blood, in relation to carbonic acid, I believe to be one of great interest and importance, and capable, when further investigated, of throwing light on many obscure parts of pathology, and especially on the immediate cause of death, and that happy absence of pain in dying which is commonly witnessed.

As regards an exhaling power, which I suppose the lungs may possess, I conceive it may be exercised occasionally under peculiar circumstances—circumstances, in the first instance, favouring an accumulation of carbonic acid gas in the blood, as undue pressure of any kind; and, in the second instance, circumstances of a different nature, connected with the removal of undue pressure, admitting thereby the excess to pass off.

The view which I have alluded to relative to the production of animal heat, is, I believe, capable of explaining very many particulars of animal temperature in different classes of animals, and both during life, in health, and disease, and in a state of hybernation, and after death. If correct, this it must necessarily do, theory being merely an expression of facts, and truths in nature being perfectly consistent.

9.—On the difference of colour of venous and arterial blood.

Of that difference of colour between venous and arterial blood, which, it appears, was first specially demonstrated by Lower,[1] at least fifty[2] years after the great discovery of the circulation of the blood by Harvey,[3] two principal explanations have been offered by

[1] Mayow, the worthy contemporary of Lower, thus notices Lower's observation of the difference of colour of the two kinds of blood, in connection with his peculiar views of its cause:

"Atque prædicta, magis adhuc ex eo confirmantur, quod sanguis, qui sub-atro colore pulmones intravit, idem magis floridus, rutilusque qualis est sanguis arteriosus, ex iisdem redit, prout *à Clariss. Lowero* in vivi-sectionibus observatum est; idemque ostendit mutationem istam in cruoris massa factam, non tam à comminutione ejusdem in pulmonibus, quam ab aere ei admixto provenire: etenim sanguinis venosi in vase excepti, superficies summa, quæ aeri exposita est colorem coccineum, floridumque acquirit; cum tamen sanguis iste in fundo vasis sub colore atro-purpureo apparet, qui tamen idem aeri expositus, post breve temporis spatium colorem rutilum induet. Ut mirandum non sit, si sanguis in pulmonibus, ubi viz. aer per particulas ejus quasque diffusus, cum eodem intimè permiscetur, per totum floridus reddatur." De sal-nitro et spiritu nitro-aëreo. cap. viii. De spiritu nitro-aereo quatenus ab animalibus hauritur, in Op. om. p. 131.

[2] The difference of colour of the two kinds of blood had been long before noticed; in Harvey's time it was a subject of controversy; he admitted the truth of the then, as now, common remark, that the blood from an artery of a living animal is more florid than from a vein, which he attributed to accidental circumstances connected with the two, maintaining at the same time, that the colour of the two kinds of blood was essentially the same, "Si sanguis è rescissâ arteriâ liberè profluens acetabulo accipiatur, venosus apparebit." He adds, "multo floridior sanguis in pulmonibus, et exinde exprimitur, quam in arteriis reperitur."

The merit of Lower consists in having associated the colour with its cause, and that not hypothetically; but experimentally, as described by Mayow, and as detailed by himself in his excellent little treatise, De Corde et de Motu et Colore Sanguinis, published in 1669. The following is his general conclusion on the point in question:—"Quare sanguinem in suo per pulmones transitu aërem haurire, ejusque admixtioni floridum suum colorem omnino debere maximè verisimile est; postquam autem in habitu corporis et viscerum parenchymatis aër rursus à sanguine magnâ ex parte avolavit, atque per poros corporis transpiravit, sanguinem venosum illo privatum obscuriorem et nigriorem illicè apparere, rationi pariter consentaneum est."— Op. cit. p. 170.

[3] In 1615, it appears, Harvey first publicly taught the doctrine of the circulation of the blood; he was then æt. 37; the discovery was probably made about the time of his return from Italy, in 1602, as he told Boyle, that he derived the first idea of it from considering the use of the valves, to which his

physiologists. By the majority, following Mayow, the bright ver-
milion hue of arterial blood has been attributed to the influence of
the oxygen of the atmospheric air inspired on the darker venous
blood, and the formation and separation, either directly or indi-
rectly, of carbonic acid. By Dr. Stevens, recently, a totally differ-
ent view has been taken of the subject. He considers the salts of
the serum more concerned in producing the florid hue of arterial
blood than oxygen, believing that they impart this colour to the
colouring matter, hematosine; that venous blood acquires its dark
colour in the extreme systematic circulation; and that it loses the
dark colour, and resumes the florid hue, in its extreme pulmonary
course; in the one instance acquiring carbonic acid, in the other
losing carbonic acid, in the latter expelled by the attraction of the
oxygen inspired.[1]

The results of the experiments which I have already given, ap-
pear to me no wise reconcilable with this theory of Dr. Stevens;
but perfectly in accordance with the older views which he contro-
verts. Of this description is the fact, that venous blood does not
acquire the arterial hue when acted on by the air-pump, even when
carbonic acid gas is disengaged; and that venous blood does ac-
quire it, from the absorption of oxygen, without the disengagement
of carbonic acid; and acquires it even when agitated with a mix-
ture of carbonic acid gas and oxygen gas, absorbing a small por-
tion of the latter, and a large portion of the former.

I apprehend, that the circumstances which Dr. Stevens has
brought forward in support of his argument, admit of explanation
consistent with the commonly received opinion of the part per-
formed by oxygen in respiration. I shall mention a few, the more
important of them, with others tending, as I believe, to elucidate the
subject.

Certain agents, as it is well known, darken the blood, whether
venous or arterial, such as most of the acids, the fixed alkalies, and
ammonia in aqueous solution, and water itself. In every instance

attention was probably specially directed whilst studying at Pavia, by Fa-
bricius ab Aquapendente, who claimed for himself the discovery of them in
the veins, and sagaciously conjectured their use in the extremities.

[1] The following passage, expressing Dr. Stevens's peculiar views, is from
his paper, entitled "Observations on the Theory of Respiration," in Phil.
Trans. for 1835.

"I have said that black is the natural colour of the colouring matter; but
when this agent is diffused in a saline fluid, such as the serum, it is of a
bright scarlet tint, which is in fact the natural colour of arterial blood.
When carbonic acid is added to this blood in the extreme circulation, it be-
comes dark red; but when this acid is removed in the pulmonary organs,
the blood then resumes its natural scarlet or arterial colour; and this, as I
have said, is produced, not directly by oxygen, but chiefly, if not entirely,
by the action of the salts of the blood on the colouring matter. Oxygen, it is
true, changes the colour from venous to arterial: this, however, is effected,
not by any specific action, but by the removal of the carbonic acid, which
had been the cause of the dark colour in the venous circulation."

in which I have examined blood so darkened, I have found more
or less of the colouring matter dissolved, and the form of the
blood-particles, as ascertained by the microscope, more or less
altered; commonly, greatly reduced in size, and from discs com-
monly converted into globules.

Certain other agents have a contrary effect, and, on immediate
admixture, apparently brighten the blood. Amongst the insoluble,
or slightly soluble substances, may be mentioned, magnesia and its
carbonate, white oxide of arsenic, boracic acid, viz. the hydrate in
its crystalline form. Amongst the soluble substances may be spe-
cified those well-known ones, the neutral salts, common salt, the
carbonated alkalies, sugar.

The first class do not appear to produce any change in the blood-
corpuscles; I have not been able to detect any. Probably the effect
is owing partly to the introduction of air mechanically entangled;
partly to the separating of the blood-particles also mechanically; and
partly to the white particles of the substances reflecting the colour
of the blood-particles interspersed.

The second class—(for instance, mixed with the blood in concen-
trated aqueous solution,) do not appear, more than the preceding, to
alter immediately the form of the blood corpuscles (this is the result
of all my trials); but to separate them from each other and prevent
their coalescing.[1] And, may it not be to this circumstance, that in
relation to colour, they owe their apparent brightening agency?

We know, that the more concentrated colouring matter is, the
darker is its hue; that the more diffused the colouring particles are,
and the more mixed with white particles; or the purer the white
ground is, on which they are spread, so much lighter and brighter
is the effect. The great masters of colouring of the 16th century
practised their art, it is understood, on these simple principles; using
always transparent colours, and never black, and so imitating the
rich mellow, and as it were living colours of natural bodies, amongst
which black is almost entirely confined to coal and charcoal.[2]

I shall give in detail a few observations in support of the preced-
ing statement. In each trial which I shall mention, the cruor of
blood was used, that is, blood deprived of its fibrin, prepared in the
manner already described.

Experiment 1.

Equal parts of the cruor of the venous blood of an ox, and water,

[1] It is well known, that when a drop of cruor (serum with blood-particles
suspended in it) taken from crassamentum, recently formed, is placed under
the microscope, the particles collect together in groups and coalesce by their
broad surfaces, forming little piles, or rouleaux—and this both in the instance
of venous and arterial blood—which coalescence (the one alluded to above)
is prevented by a saline solution.

[2] These remarks are applicable to hematosine, which is apparently black
only in mass; in powder, or seen in small pieces by transmitted light, it is
red.

were mixed together. The colour of the mixture was darker than that of the cruor alone, especially by reflected light. Under the microscope, no blood discs could be discovered in the mixture—only small globules, about one-third of the diameter of the discs, of faint appearance, as if transparent. Solution of salt added to the mixture rendered it of a brighter red—light scarlet. Now, under the microscope, the particles appeared smaller, and well defined, and distinct, so that they were most easily seen. The distinctness of their appearance was not increased by the addition of corrosive sublimate.[1]

In similar trials with solution of ammonia, of potassa, of hydrocyanic acid and strong acetic acid, the effects on the blood seemed to be very similar; the appearances were much the same, whether viewed with the naked eye or with the microscope; the colouring matter was dissolved, the forms of the discs were changed, and a darkening effect was produced.[2]

Experiment 2.

Equal parts of a strong solution of refined sugar and cruor were mixed together. The mixture immediately appeared of a brighter hue; at and near the upper surface scarlet; towards the bottom light brick-red. After twenty-four hours, it appeared generally of a scarlet hue, until it was agitated, when the interior and central portion of the mixture was found to be dark. Under the microscope the blood-discs were perceived to be unaltered. The effect of admixture of common salt, of nitre, of muriate of ammonia, of carbonate of potash, was very similar; as viewed with the naked eye and with the microscope. After twenty-four hours, in the instance of common salt and nitre, the solution was hardly tinged by the colouring matter of the blood, and the blood-corpuscles remained distinct in form—near the surface, of a vermilion hue—within of a darker hue. In the instance of muriate of ammonia and carbonate

[1] When a portion of crassamentum is put into water, its hue is presently darkened; and the same effect is produced by allowing water to run over the crassamentum. Dr. Stevens is of opinion that this is owing to the removal of the saline matter by the water. According to my observations, in conformity with those stated in the text, it is occasioned by a change in the blood-corpuscles themselves, and a partial solution of the colouring matter. When the darkened crassamentum is taken from the water and put into a solution of salt, it immediately acquires a brighter hue. This Dr. Stevens refers to the restoration of the saline matter. I apprehend it is owing to the effect of the saline matter chiefly on the altered blood-corpuscles—by which they reflect more light.

It is surprising how slowly the colouring matter is removed from the crassamentum by water, when the mass is not broken up; the water appears to penetrate very slowly through it; and the colouring matter, even when dissolved, seems to be retained in the mass as in a sponge.

[2] In all these instances, the solution of the colouring matter, viewed by transmitted light, was rich red, approaching the ruby hue; in particular instances, compared one with another, there were slight differences of colour.

of potash, a little of the colouring matter was dissolved, and the colour of the mixture was less bright, and the blood-discs were a little smaller.[1]

Experiment 3.

A little muriatic acid and cruor were mixed together, the former not in sufficient quantity to coagulate the serum. The mixture presently acquired a darker hue. Under the microscope, the blood-corpuscles appeared somewhat altered in form—they were less distinctly disc-like—they approached more to the globular, and had minute particles (probably of air) adhering to them. On the addition of more and of stronger acid—effecting the coagulation of the serum—the blood-corpuscles were found diminished in size and irregularly globular; they were examined in a state of dilution by the addition of a little saturated brine.

Do not these results warrant the preceding inference? All the facts with which I am acquainted appear to me to indicate that the colour of the blood, whether venous or arterial, that is, dark or florid, is independent of the saline matter in the serum, considered in relation to agency; and that according to the commonly received view, oxygen is the cause of the bright hue of the arterial fluid—and its consumption and conversion into carbonic acid, the cause of the dark hue of the venous—the saline serum being negative in regard to colour, its chief use being to preserve the red globules from injury, prevent the solution of their colouring matter, retain their forms unchanged, and to bear them in their course through the circulation.[2]

Why oxygen should render the colouring matter of the blood scarlet; why its conversion into carbonic acid should change the scarlet to a darker hue; and what is the true shade of colour, of the colouring matter of the blood, are questions, to which, I believe, the present state of our knowledge does not admit of a reply in a satisfactory manner. The colouring principle of the blood-corpuscles, in its perfect state, can hardly be considered precisely the same as the hematosine extracted from them; the former is not soluble in serum—the latter is; nor have I found the colour of the latter in

[1] I have not found an aqueous solution of hematosine brightened in colour by common salt. It contains no globular particles suspended in it, like the mixture of cruor and brine, to enable the effect to be produced—unless the common salt acted chemically, which it does not appear to do.

[2] The conclusion relative to the colour of the blood—and especially its modification by neutral salts, acids, and alkalies, and other agents, to which my experiments have led me, is very similar to that of Dr. Wells, deduced by him from a somewhat different train of experiments and reasoning, more than forty years ago, as described in his very ingenious paper, entitled "Observations and Experiments on the Colour of the Blood," published in the Philosophical Transactions for 1797. This paper, which has been much overlooked of late years, is every way worthy of the very original mind of its author, and will, even now, well repay the perusal.

solution affected by oxygen; consequently, we cannot, with advantage, predicate the properties of the one from the other.

10.—Do the arteries after death contain air?

Harvey, from the results of his researches, came to the conclusion that the arteries after death, as well as during life, are destitute of air.[1] This too, was the conclusion of Prochaska; he opened the arteries under water, and could find no air in them.[2] Amongst very recent inquirers, Professor Burdach is, I believe, peculiar for maintaining an opposite opinion. He says, "Les arterès des cadavres sont vides de liquide, et par conséquent pleines d'air, qui doit s'être dégagé du sang."[3] In support of this inference, he refers to the observations of Redi and Caldesi, of Haller, Spallanzani, and of Blumenbach, according to which bubbles of air are not uncommon in the blood of reptiles, even when circulating in their vessels; he adduces the fact of the readiness with which air is absorbed by the blood; and, the additional fact, as he believes, of blood readily giving off air under the exhausted receiver. These are the circumstances on which he founds his opinion, and comes to the conclusion, "Si Prochaska n'a point vu de bulles s'en élever lorsqu'il les ouvrait sous l'eau, il faut que quelque erreur d'observation se soit glissée dans cette experience."

Theoretically viewed, especially taking into account the great difficulty with which air is separated (when it is possible) from the blood, by the air-pump, and never, as far as I have observed, in its vessels—this conclusion of Professor Burdach is open to many doubts; doubts which are not removed by his after reflections.

The question is necessarily one of difficulty, as well as of much physiological interest in relation to its bearings; and, of a nature, as

[1] The passages from Harvey bearing on this, are in many respects interesting and instructive. Discussing the question which was agitated amongst the ancients, whether the arteries are the channels of air or of blood, he remarks, "Sanguinem in arteriis contineri, et arterias solum sanguinem deferre tum experimento Galeni, tum in arteriotomia, tum in vulneribus manifestum est: cum ab una arteria dissecta, hoc etiam Galenus affirmat, plurimis in locis, unius semihorae spatio totam massam sanguinis ab universo corpore, magna et impetuosa profusione exhaustam fore. Experimentum Galeni tale est: Si (inquit) funiculo arteriam utrimque ligaveris, et medio rescisso secundum longitudinem, quod inter duas ligaturas in arteria comprehensum erit, nihil præter sanguinem reperies: et sic probat sanguinem solum continere. Unde etiam similiter nobis ratiocinari licet: si eundem sanguinem, qui venis similiter ligatis, et rescissis inest, inveneris in arteriis (quem admodum in mortuis, et aliis animalibus sæpius ego expertus sum) eâdem ratione similiter concludere nos possumus, arterias eundem sanguinem, quem venæ, et nihil præter eundem sanguinem continere." Exerc. Anat. Proem.

[2] Disquisitio Anatomico-physiologica organismi corporis humani ejusque processus vitalis, p. 87.

[3] Traité de Physiologie considerée comme Science d'Observation. Traduit par A. Jourdan. Tom. vi. p. 285.

it appears to me, admitting of solution only by accurate and strict observation independent of speculative views. If it is a fact, that the arteries contain air after death, and that air is carbonic acid, it will be easy to account for it by the result adverted to in the preceding 8th section. To endeavour to determine this point, as regards matter of fact, I have availed myself of the opportunities which have offered in the general hospital at Fort Pitt, to institute on the cadaver the examination necessary. The inquiry was begun in the month of September, 1838; and in every fatal case that afterwards occurred, with the exception of four, during a period of six months, search was made for air. As most convenient for examination and best fitted to give accurate results, the internal jugular vein (thinking it advantageous to extend the inquiry to this vessel) and the carotid artery in the middle of the neck, were selected for the purpose. In every instance the vessels were exposed with caution—as much care as possible being taken to avoid the wounding of branches. To determine if any air was in the vein, not satisfied merely with the appearance of the vein, a puncture was always made into it, and some blood pressed out : and, to determine the same of the artery, a portion of it, about an inch in length, where it gives off no branches, was included between two ligatures; cut out so secured ; and transferred to water and there opened. The description of cases in which the trials were made, is shown in the two following tables, in which also are noticed some other particulars, which for the sake of accuracy it appears desirable to specify.

. The results of the examination in the nineteen cases included in the first table, were, as far as the artery was concerned, entirely negative; not a single particle of air could be observed, in any instance, to escape from the vessel, opened under water. The artery, in every case, was collapsed; flattened; its upper and under surface in contact, from atmospheric pressure,[1] and either quite destitute of blood, or it contained only a very minute portion, in the space of an inch, never equal to a single drop. And, in the instances of the vein, with one exception, that of case No. 3, the result was similar, strongly confirming the absence of air.

The trials, the results of which are given in the second table, were made for the sake of comparison. I expected, that in every instance, air would be found in the artery as well as in the vein. The circumstance, that in three out of the eight cases in which the experiment was made, no air could be procured from the artery opened under water—shows (to express the fact in popular language for the sake of brevity) how feeble is its power of suction ; and how very inadequate, on the principle of removal of pressure, to draw air from the blood, according to the idea involved in the hypothetical view of Professor Burdach.

[1] When opened, either in the air or under water, in every instance, it immediately recovered its cylindrical form, air or water entering.

TABLE 1.

Cases in which the Carotid Artery and Jugular Vein were examined for air, before the removal of the Calvaria.

No	Age.	Fatal disease as determined by autopsia.	Time of death.	Time of examination (in hours) after death	REMARKS.
1	26	Pulmonary hemorrhage	Sept. 17	12	Vein distended with cruor.
2	45	Inflammation and suppuration of joints	,, 19	1	Vein distended with liquid blood, which quickly coagulated on exposure.
3	23	Phthisis pul.—pneumothorax	,, 24	18	Vein distended with cruor, and contained two or three minute bubbles of air.
4	37	Cerebral hemorrhage	,, 25	12	Vein distended with cruor.
5	31	Ascites: indurated spleen and liver	Oct. 7	4½	Vein distended with fluid blood; when opened, it flowed in a jet, it may be inferred, from abdominal pressure; it afterwards coagulated.
6	32	Peritonitis	,, 7	17	Very little blood in vein.
7	31	Phthisis pul.	,, 10	19½	A little cruor in vein.
8	32	Chronic dysentery	,, 8	8	A minute quantity of blood in vein.
9	19	Phthisis pul.	,, 11	1½	A very little fluid blood in vein, which speedily coagulated.
10	34	Empyema—pneumothorax	,, 18	4	A little very dilute cruor in vein.
11	41	Phthisis P.—pneumothorax	,, 22	5½	Vein moderately distended with liquid blood, which, let out, presently coagulated.
12	38	Phthisis—pneumothorax	Nov. 7	20	Vein empty.
13	22	Pleuritis—peritonitis	,, 9	12	Vein moderately distended with cruor which did not coagulate.
14	37	Pulmonary gangrene	,, 16	20	Vein empty; after removal of calvaria some air in the other jugular vein; none in carotid artery.
15	26	Phthisis pulmonalis	Dec. 3	11	A little liquid blood from vein, which presently coagulated, and had a buffy coat.
16	27	Ascites; œdema of glottis; bronchitis	,, 5	2	Vein distended; about 1lb. of blood flowed from it, when opened, coagulated feebly.
17	36	Aneurism of aorta—indurated lung	,, 18	3	Vein moderately distended; coagulated when let out, without buffy coat.
18	61	Malignant tumour	,, 29	20	A little blood from vein which did not coagulate.
19	33	Ruptured aneurism into trachea	Feb. 27	1½	Blood from vein presently coagulated without buffy coat.

TABLE II.

Cases in which the Carotid Artery and Jugular Vein were examined for air, after the removal of the Calvaria.

No.	Age.	Fatal disease as determined by autopsia.	Time of death.	Time of examination (in hours) after death.	REMARKS.
1	18	Phthisis pulmonalis	Nov. 14	11	Several large bubbles of air in vein; two minute bubbles in artery.
2	20	Phthisis pulmonalis	Jan. 3	14	Air-bubbles in vein; none in artery.
3	21	Phthisis pulmonalis	,, 9	33	A little air in vein and artery.
4	29	Phthisis pulmonalis	,, 11	11	Some air in vein; very little in artery.
5	18	Empyema	,, 11	33	A little air in vein and artery.
6	26	Phthisis pulmonalis	,, 24	27	A little air in vein and artery.[1]
7	20	Phthisis pulmonalis	,, 30	28	Some air in vein; none in artery.
8	20	Diabetes mellitus & phthisis pulmonalis	Feb. 13	27	A little air in vein; none in artery.

[1] In this instance the thorax and abdomen, as well as calvaria, were opened before the vessels were examined for air.

As the results, which I have brought forward without selection, are perfectly in accordance with the preceding observations of Harvey and of Prochaska—may it not be fairly inferred, that their conclusion is the true one; that the arteries, commonly after death, do not contain air; and that when they differ from this their normal state, it must be presumed to be the effect of peculiar circumstances, requiring special invstigation.

Whether the arteries, after death, usually contain something intermediate between fluid blood and elastic air, appears to me questionable in a very high degree. The distinguished physiologist last mentioned, appears to be of opinion, that they may, and that they partly owe to the vapour, which he infers they may contain, certain qualities peculiar to their vessels, particularly the manner in which they contract and shorten when divided.

IV.

OBSERVATIONS ON THE BLOOD AFTER DEATH.

For many years past, in the *post mortem* examinations of bodies, I have been in the habit of paying attention to the condition of the blood in the heart and great vessels, especially in relation to coagulation; and latterly, in conjunction with coagulation, to the effects of agitation on the blood in common air, as a test, whether or no it contain carbonic acid gas in excess, admitting of being extricated by this method.

I shall first give the general results of my observations, made in Malta, from the year 1828 to 1835, as an example of the variations in the appearances of the blood after death, and of their comparative frequency. For the sake of brevity I shall present them in a tabular form.—[See table on next page.]

The fatal cases, affording the subjects of these observations were, without exception, soldiers of our regiments, serving in Malta, composed of Englishmen, Scots, and Irish.

In the cool season, the bodies were commonly inspected in from twelve to seventeen hours after death; in the hot season, in from three to twelve.

As the table is brought forward chiefly as an axample, intended to show how various is the condition of the blood after death, I shall restrict myself to a very few remarks on the different appearances of the blood noticed, and especially in connection with the fatal diseases. To do justice to the subject, it would be necessary to enter into very minute details, and give a statement of particulars, which could be collected only by laborious research—and which, at present, I am not all prepared to offer.

TABLE I.

Of the Appearances of the Blood after Death from various Diseases.

Year.	Total number of fatal cases examined.	Heart collapsed, containing either no blood or a very minute quantity.	Blood liquid; not coagulating on exposure to the air.	Partly liquid, partly coagulated—the former not coagulating on exposure.	Partly liquid, partly coagulated—the former coagulating on exposure.	Liquid and coagulating on exposure.	Coagulated and containing fibrinous concretions.	Coagulated, without fibrinous concretions—the coagulum soft.	Grumous, not distinctly coagulated, without fibrinous concretions.	Liquid and frothy—without any fibrinous concretions.	Blood florid.	Fibrinous concretions without any cruor.	Blood coagulated in both ventricles of heart, and broken up.	Coagulated and broken up in left ventricles, no; in right.	Coagulated in right ventricle and broken up; left ventricle empty.	Cases in which the state of the blood was not noticed.
1828	28	—	2	2	2	1	21	—	—	—	—	—	—	—	—	—
1829	18	2	—	—	2	—	14	—	—	—	—	—	—	—	—	—
1830	36	—	1	1	1	—	18	—	2	1	1	—	—	1	—	11
1831	54	2	1	—	—	—	21	3	—	—	—	—	3	5	—	19
1832	29	2	2	—	—	—	8	1	—	—	—	—	1	—	1	14
1833	35	—	2	—	—	1	7	3	—	—	—	—	1	4	—	17
1834	40	—	1	—	1	2	14	2	—	—	—	1	—	1	—	17
1835	9	—	—	—	—	—	2	—	—	—	—	—	—	—	—	7
Total	249	6	9	3	6	4	105	9	2	1	1	1	5	11	1	85

1. The few instances in which the heart was found empty, or containing only a very minute quantity of blood, were chiefly cases of death from ruptured aneurism.

2. The instances in which the blood was found liquid and not coagulable on exposure to the air, the fatal event, in the majority of cases, was referable to the respiratory organs, and was owing either to drowning, hanging, the fumes of a charcoal fire, or the effusion of blood into the bronchia or air-cells. The former occurred in a case of pneumonia; the latter in two cases of sudden death, from what Laennec has called pulmonary apoplexy. The blood's liquidity in these instances was not a *post mortem* effect, resulting from putrefaction; this was clearly ascertained. It was owing either to a loss of the natural fibrin or lymph, on which the property of coagulation depends, or such an alteration in it, as to deprive the lymph of this quality. In one instance, a few days previous to death, some blood abstracted from the patient, exhibited in relation to coagulability its normal condition.

3. Relative to the condition of the blood noticed in the 4th and 5th column, as either liquid—coagulating on exposure, or partly liquid and coagulable, I believe, it depended (whether one or the other) on the time after death, when the *post mortem* examination was made: I shall have occasion further on, to bring forward examples in illustration of this remark.

4. Of the 164 cases in which the state of the blood was specially noticed, in 105 instances it was found coagulated, the coagulated blood mixed with fibrinous concretions, or coagulated lymph, the material of the buffy coat; and I believe it was found in the same state in the majority of the 85 cases, in which its condition was not specified.

5. The instances in which the blood was found coagulated and without fibrinous concretions (the coagula soft) were chiefly cases of tubercular phthisis. In the majority of the fatal cases, however, of this disease, fibrinous concretions occurred.

6. In one instance in which the blood was found grumous, not distinctly coagulated, death was owing to confluent variola; the lungs were hepatized. In the other instance, the disease was returned debility; it was of a very obscure nature, and not connected with any well marked organic lesion. Whilst grumous blood was found in the cavities of the heart in this case, fibrinous concretions were met with in the great vessels.

7. In the solitary case in which the blood was found liquid and frothy, and did not coagulate on exposure, putrefaction was far advanced; and it is probable that the fibrin had become liquid after coagulation in consequence of putrefaction. In this instance all the parts in contact with blood were strongly stained by the colouring matter of the blood in a state of solution in the serum. The phenomenon of the red stain, as a *post mortem* effect, is, I believe, invariably connected with this condition of the serum, and which itself is connected more or less with putrefaction. I have never

yet met with an exception to this remark; wherever I have found red serum, on microscopical examination, I have detected change in the form of the red particles, indicative more or less of decomposition.[1]

8. The single case, in which fibrinous concretion occurred in the heart without crassamentum or cruor, was one of dysentery.

9. The single case in which the blood was observed florid, was an instance of tabes mesenterica, without tubercles in the lungs. The mesenteric and lumbar glands were vastly enlarged,—some of them the size of pigeons' eggs, and contained a curd-like albuminous matter; and tubercles containing a similar matter existed under the mucous coat of the transverse colon. The blood in this instance was coagulated; its florid hue was most strongly marked in the aorta.

The broken-up state of the coagulated blood referred to in the three last columns of the table, to the best of my knowledge, has not yet been described by any pathological writer. The appearance exhibited was as if one or both ventricles of the heart had continued

[1] Most animal textures, chemically considered, have a strong affinity for the colouring matter both of blood and bile,—and for the former especially when in solution, that is, separated from the blood-corpuscles, whether in water or serum. In a paper communicated to the Medico-Chirurgical Society of London, in 1818, and published in its Transactions,—I pointed out certain false appearances of inflammation, *post mortem* effects, connected with this quality. I then stated, "these false appearances are in many instances so exactly like the true, that I doubted if the most experienced anatomist would be able to distinguish between them:" adding, "that I had made the trial with medical friends by asking their opinion of parts coloured by immersion in blood, and, without the least hesitation they had pronounced the parts strongly inflamed:" afterwards in Malta, I continued the inquiry, and in 1828, made a series of experiments on the colouring power of blood and bile on the different textures of the body. The principal results were, that the stains of both are with difficulty removed by washing, and from several textures, even by maceration; and that by mere colour it is impossible to distinguish between the *post mortem* chemical effect,—and the effect, during life, of inflammation. The influence of temperature is of the first importance in relation to the production of bloody serum,—that is, serum holding in *solution* the colouring matter of the blood. As a temperature between 80° and 100°, the solution will take place in a few hours, and the staining will be produced,—the pseudo-inflammatory appearances will present themselves: on the contrary, at a temperature between 35° and 45°, the blood-corpuscles will retain their colouring matter and remain unaltered, and the serum untinged for many days, and so long none of the false appearances of inflammation alluded to will be observable. Another point, of much importance, is the degree of tendency of the body after death to putrefy. In some instances, even before death, there appears to be incipient putrefaction of the blood; and, in these cases, bloody serum is detected accompanied with staining of the parts with which it is in contact, though the *post mortem* examination may be made before the cadaver has had time to become cold. In other instances,—and they may occur at the same time, the tendency to putrefaction is so very inconsiderable, that no indications of it are perceptible, even after twenty-four hours, and sometimes longer; and in these examples there is a corresponding absence of any false appearances of inflammation.

to act after the coagulation of the blood had taken place within them. On no other idea can I account for the phenomenon. I never witnessed it in the blood-vessels, or even in the auricles, which perhaps may be considered in accordance with the supposition respecting its cause. Of the seventeen instances in which this condition of the blood was detected, death in the majority was owing to tubercular phthisis, viz. in 11 ; in two, the fatal disease was pneumonia, the lungs extensively hepatized ; in one, abscess of the lung accompanied with gangrene ; in one, chronic dysentery accompanied with abscess in the liver ; and in one the death was from violence, a soldier in good health was shot through the head by a comrade. In the majority of the instances, fibrinous concretions accompanied the broken-up crassamentum.

I shall now proceed, and briefly notice some later observations on the blood after death, made in the general hospital, at Fort Pitt, during a period of nine months, viz. from January to September, 1838, in which attention, as before mentioned, was paid not only to the state of the blood in relation to coagulation, but also to its state in relation to carbonic acid gas.

The method employed was briefly the following : In every instance, after the removal of any fluid that might have been found in the pericardium, the great vessels within it were divided, the blood which flowed out was collected and put into a bottle and secured with a glass stopper for after-observation and experiment. The cavities of the heart and their contents were next examined. Sometimes the blood set apart was seen again the same day, to ascertain whether it had coagulated or remained liquid ; but more frequently, not until the following morning, when also commonly it was subjected to agitation in atmospheric air included in a double-mouthed bottle provided with stop-cocks, to one of which was attached a bent tube for communication with a pneumatic trough.

I shall give the results in a tabular form individually, with the age and fatal disease, the day of the month of its termination, the interval between the time of death and *post mortem* examination, and between the latter and the examination of the blood,—circumstances without which little value can be attached to the record ; and under the head of Remarks, will be appended the organic lesions discovered, of most importance, characteristic of the disease, with occasional notices of lesions of rare occurrence.

I may premise further, that in using, for the sake of brevity, the term " cruor," I have not employed it to designate merely a mixture of serum and red particles, but a mixture of the two in which there may be some coagulable lymph, to be determined by trial. The terms " crassamentum" and " clot" I have used synonymously, in their ordinary acceptation, as applied to the blood. The term, " fibrinous concretion," I have employed to designate masses of lymph or fibrin, in a coagulated state, such as used to be called polypi.

TABLE II.

Of the condition of the Blood after Death from various Diseases, and its Changes.

No.	Age.	Fatal disease determined by dissection.	Time of death.	Post mort. inspection (hrs. after death.)	General condition of the blood in the heart.	After condition of the blood set apart.	Results of agitation of the blood or cruor.	Remarks relative to morbid appearances.
1	19	Pericarditis	Jan. 20	16	Pretty much cruor and a little clot in right cavities of heart: no fibrinous concretions.	-	-	Agitated immediately: absorbed a little air: and again the following day.	Pericardium nearly an inch thick from deposition of lymph: contained 20 ounces of purulent serum. Granular tubercles in the lungs.
2	29	Phthisis pulmonalis	" 26	18	About three ounces of cruor and dark crassamentum in right cavities of heart.	Two hours after, the cruor found coagulated bly.	Immediately after coagulation gave off some air on agitation. On 1st Feb. had an offensive smell: and on agitation absorbed a little air.	The lungs abounded in tubercles and vomicæ: the left pleura contained 46 ounces of purulent serum, which, on agitation, gave off a good deal of air.
3	22	Rubeola	Feb. 3	28	Much dark liquid blood in right cavities of heart.	-	-	Agitated about two hours after, gave off some air: agitated a second time, immediately there was slight absorption: rubbed with quick lime, a slight ammoniacal odour.	Bronchia dark red: engorgement of pulmonary vessels, and some ecchymosis in air-cells: death on fourth day.
4	18	Phthisis pulmonalis	" 19	29	Much cruor with fibrinous concretions in right cavities.	Six ounces of cruor collected, found liquid 20 hours after.	Agitated 20 hours after, gave off about half cubic inch of air.	Lungs abounding in tubercles and small vomicæ: partially hepatized: partially œdematous: a portion of large vein in right lung to the extent of about half an inch plugged up with firm fibrin.

TABLE II.—*Continued.*

No.	Age.	Fatal disease (determined by dissection.)	Time of death.	Post mort. inspection hrs. after death.	General condition of the blood in the heart.	After condition of the blood set apart.	Results of agitation of the blood or cruor.	Remarks relative to morbid appearances.
5	19	Bronchitis	Feb. 23	33	Much cruor and fibrinous concretions in right cavities: the former of sp. gr. 1045.	Cruor did not coagulate.	Cruor agitated on 28th Feb. gave off no air: three days later it gave off some air: it had then a putrid smell.	The bronchia smeared with pus: an ulcer on upper surface of epiglottis: two small clusters of granular tubercles in lower part of superior lobe of right lung.
6	20	Phthisis pulmonalis	Mar. 22	30	A good deal of cruor and fibrinous concretions in right cavities.	Six ounces of cruor collected, on following day slightly coagulated.	On agitation, after having been broken up, a very slight absorption: agitated with carbonic acid, it absorbed 116 per cent.	Cavities and tubercles in both lungs.
7	24	Chronic pericarditis	" 30	14	Some cruor, soft crassamentum,—and a little fibrinous concretion in right cavities.	The following morning the cruor was found or was feebly coagulated.	On agitation pretty much air disengaged: and also from the serum.	Seven pounds of serum in cavity of abdomen: thickening of the peritoneal covering of most of the viscera: a few clustered granular tubercles in lungs: tubercles in kidneys, prostate and testes: and loose putty-like matter in left vesicula seminalis.
8	70	Chronic peritonitis, with abscess and gangrene of lungs.	Apr. 13	48	Only about three ounces of blood (crassamentum) from the heart without fibrinous concretion.	-	Clot broken up and agitated: there was a slight absorption of air.	A small gangrenous abscess in right lung: no tubercles in either lung: 16 pints of serum in cavity of abdomen: some pus in cavity of pelvis. Liver weighed only 2lbs.: liver and pancreas unusually firm. The serum agitated the following day gave off a good deal of air: the patient for many years had laboured under amentia.

TABLE II.—*Continued.*

No.	Age	Fatal disease determined by dissection.	Time of death.	Post mort. inspection (hrs. after death.)	General condition of the blood in the heart.	After condition of the blood set apart.	Results of agitation of the blood or cruor.	Remarks relative to morbid appearances.
9	59	" "	May 3	26	Cruor and soft coagula in right cavities of heart.	Three ounces of cruor collected, the following morning jellied.	On agitation, after having been broken up, gave off a good deal of air.	Blood effused on left hemisphere of cerebrum on the arachnoid membrane: fornix soft. Inferior lobe of left lung œdematous, contained two cavities in which a puriform fluid: no tubercles. A patient in Lunatic Asylum labouring under amentia.
10	20	Phthisis pulmonalis	" 5	22	Pretty much cruor and fibrinous concretions in cavities of heart.	The cruor slightly coagulated the following morning.	Broken up, a good deal of air given off on agitation.	Many clustered granular tubercles, and two or three small cavities in superior lobe of left lung: several clusters of granular tubercles in superior lobe of right lung: right lung extensively hepatized: 8 ounces of purulent serum in right pleura: 3 ounces in left: 2 ounces in præcardium.
11	21	Œdema glottidis	" 6	21	A good deal of dark liquid blood in the large vessels, and in right cavities of heart: a small portion of fibrin in right ventricle.	After three hours continued liquid.	On agitation gave off a good deal of air.	Unexpected and almost sudden death from œdema of glottis, margin of epiglottis and sacculi laryngis: the uvula was also œdematous.
12	20	Bronchitis chronic phthisis pulmonalis	" 17	48	A good deal of cruor and fibrinous concretions in cavities of heart.	-	On agitation immediately gave off some air.	Bronchia red, and contained muco-purulent fluid: under-surface of epiglottis superficially ulcerated: an ulcer at base of right arytenoid cartilage: numerous granular tubercles in both lungs: a small vomica in superior lobe of right, and three in the superior lobe of left lung. The body little emaciated.
13	29	Phthisis pulmonalis	" 17	45	Some cruor and fibrinous concretions in right cavities of heart.	-	On agitation immediately gave off no air: there was no absorption.	Lungs abounded in tubercles, and contained several cavities: large intestines severely ulcerated. Died suddenly in the act of speaking.

14	31	Phthisis pulmonalis	May 25	27	A good deal of coagulated blood, cruor, and fibrinous concretions in right cavities of heart.	The cruor put aside was liquid the following morning.	On agitation gave off a little air.	Large cavities and numerous tubercles in the lungs: lungs partially hepatized: in the right ventricle of heart were two masses of fibrin closely adhering, in each of which was a puriloid fluid, consisting (as it appeared under the microscope) of a few blood discs, globules very like those of pus, and of other particles less regular, and of various sizes.
15	31	Pneumonia	" 21	26	A good deal of cruor, coagulated blood and fibrinous concretions in cavities of heart.	Cruor liquid the next morning.	On agitation gave off some air.	Some œdema of left lung, weight three quarters of a pound: right lung extensively hepatized, of a fawn colour: it weighed four pounds: a slight deposition of lithate of soda on cartilages of tibiæ and of first joint of great toes. It was reported that he had been subject to rheumatism.
16	28	Phthisis pulmonalis	June 1	12	Some crassamentum, fibrinous concretions and a good deal of cruor in right cavities of heart.	Two hours after, the cruor was found jellied.	After twenty-four hours the coagulum had contracted: serum had separated: broken up and agitated, some air given off.	Cavities and numerous tubercles in lungs: severe ulceration of larynx: in a mass of fibrin in the right ventricle was a collection of transparent serum: the mass was firmest externally.
17	34	Aneurism	July 21	26	Six ounces of thin cruor in right pleura, with a very small portion of clot.	The cruor did not coagulate.	Agitated twenty-four hours after, gave off a very little air.	A false aneurism close to cœliac artery, with two circular openings from aorta within half an inch of each other: the sac having burst, a large quantity of blood was poured out and coagulated in omentum, &c.: a portion entered the posterior mediastinum and ruptured it.
18	39	Phthisis pulmonalis	" 24	26	A good deal of dark cruor and fibrinous concretions in right cavities of heart: a mass of fibrin in left ventricle.	The cruor (six ounces) was found jellied the following day.	Broken up and agitated it gave off a little air.	Several cavities in lungs, and numerous tubercles with some hepatization.

10—d

8 day

TABLE II.—*Continued.*

No.	Age	Fatal disease (determined by dissection.)	Time of death.	Post mort. inspection (hrs. after death.)	General condition of the blood in the heart.	After condition of the blood set apart.	Results of agitation of the blood or cruor.	Remarks relative to morbid appearances.
19	36	Phthisis pulmonalis	July 29	27	Some cruor and fibrinous concretions in right cavities of heart and in left auricle.	After twenty-four hours cruor slightly jellied.	On agitation gave off a very little air.	Tubercles abounded in lungs with cavities, partial œdema and partial induration.
20	24	" "	" 31	37	Some cruor, coagulated blood and fibrinous concretions in right cavities of heart and in left auricle.	-	Agitated immediately, pretty much air disengaged.	Right lung weighed three and a half pounds, partially hepatized, contained cavities and abounded in tubercles: left lung similarly diseased, in a minor degree.
21		Hypertrophy of heart	Aug. 1	32	A good deal of cruor in right cavities of heart and some clot.	-	Agitated immediately, gave off a good deal of air.	Heart nearly triple its natural size: ventricles most enlarged, especially left: aorta much dilated and diseased, an aneurismal pouch at its base: albuminous matter of the appearance and consistence of soft putty in left tunica vaginalis testis, left vesicula seminalis, in cavities in substance of kidneys, in ureter of the same side, and in right vas deferens; the left ureter was in several places impervious: partial œdema of lungs: no tubercles.
22	33	Phthisis pulmonalis	" 5	4	Much liquid blood in cavities of heart: some crassamentum: no fibrinous concretions.	Sixty-three ounces collected, which coagulated in a few minutes.	Broken up the following day, on agitation it gave off pretty much air.	Large excavations and numerous tubercles in lungs: death probably from suffocation, a vomica bursting and pouring its contents into bronchia.
23	46	Hydrothorax	" 4	29	A little cruor and coagulated blood in right cavities of heart.	About one ounce only could be collected: the following day cruor or coagulated.	Quantity too small for trial for air.	54 ounces of serum in right pleura, with flakes of lymph: 18 ounces in left pleura: 5 ounces in pericardium, without deposition of lymph: the serum agitated gave off pretty much air: granular tubercles in lungs and in right pleura: 32 onnces of serum in cavity of abdomen: were all the blood in the body collected, it would not have exceeded, it is probably, 4 ounces.
24	22	Phthisis pulmonalis	" 9	12	Pretty much cruor and fibrinous concretions in right cavities of heart: very little in left.	Cruor did not coagulate.	The following day on agitation, gave off pretty much air.	27 ounces of sero-purulent fluid in left pleura: lined with false membrane: cavities and tubercles in both lungs: a mass of softened fibrin in right ventricle, which it may be inferred existed before death: a mass of firm fibrin in left ventricle of heart.

25 25 Lumbar abscess	Aug. 22	"	14	A good deal of cruor in right cavities of heart: a little soft fibrinous concretion.	Cruor found coagulated the following morning.	Broken up and agitated, gave off pretty much air.	Pus in bronchia of left lung: a portion of its inferior lobe hepatized, another portion œdematous: no tubercles: lumbar vertebræ and right os ilii extensively carious.
26 28 Pneumonia	"	5	19	A good deal of cruor, erassamentum and fibrinous concretions in heart.	Two hours after removal, cruor coagulated and covered with a thin buffy coat.	Broken up and agitated the following morning, gave off no air: nor absorbed any.	Right lung weighed 3½ lbs.: its superior lobe completely hepatized: some of its vessels filled with lymph: left lung in part œdematous: no tubercles. The liver very large, not fatty, weighed 13½ lbs.: 8 oz. of serum of sp. grav. 1010, without any lymph.
27 47 Phthisis pulmonalis	"	14	24	Some cruor, a good deal of erassamentum and fibrinous concretions in right cavities of heart,—some fibrinous concretions in left ventricle.	Cruor, the following day found jellied.	Broken up and agitated, a little gas was disengaged.	Both lungs contained numerous tubercles and some cavities: arch of aorta considerably enlarged: two masses of fibrin adhered to it internally mid-way, about the size of filberts, softening but not pultaceous.
28 35 Medullary Tumour	"	17	5	Some cruor and a good deal of erassamentum in right cavities of heart.	The cruor, the following morning, pretty firmly coagulated	Broken up and agitated, it gave off a little air.	A considerable portion of the left side of face destroyed, with the greater part of corresponding maxillary bones: a tumour about the size of a lemon over the left parotid gland, its central part soft, reddish and blood-shot: composed of particles which, under the microscope, appeared to be of different sizes; some of them globular.—Viscera tolerably sound.
29 40 Dysenteria chronica	"	13	4½	Some cruor and fibrinous concretions in right cavities of heart.	After 2 hours, the cruor was found coagulated, and a slight buffy coat on it.	Broken up and agitated the following day, no air given off: a very slight absorption.	Large intestines severely ulcerated: a mass of fibrin in longitudinal sinus softening internally.
30 26 Phthisis pulmonalis	"	19	16	Some cruor in right cavities of heart,—no clot or fibrinous concretion.	Cruor liquid the following day.	On agitation did not give off any air: no apparent absorption.	Vomicæ and numerous tubercles in lungs: appendicula vermiformis distended with purulent fluid: ulcerated: its opening into cæcum perfectly closed by adhesion, (an example of mucous surfaces adhering by the effusion of lymph.)

TABLE II—Continued.

No.	Age	Fatal disease (determined by dissection.)	Time of death.	Post mort. inspection hrs. after death.)	General condition of the blood in the heart.	After condition of the blood set apart.	Results of agitation of the blood of cruor.	Remarks relative to morbid appearances.
31	2	Phthisis pulmonalis	Aug. 19	18	Some cruor and fibrinous concretions in right cavities of heart.	The following morning the cruor or slightly jellied.	Agitated, gave off a good deal of air.	Numerous tubercles in lungs, and many minute vomicæ : disease of bone of right upper extremity : portion of ulna necrosed : cartilage of head of humerus and glenoid cavity destroyed : pus in capsule.
32	10	"	" 22	8½	Some cruor,—a considerable quantity of crassamentum and some fibrinous concretions in right cavities of heart.	The cruor after two hours jellied.	Agitated, it gave off a little air.	Left lung very small, many tubercles in it, and a cavity communicating with pleura : left pleura contained 57 oz. of turbid serum, and a good deal of air : vomicæ and tubercles in right lung : the serum from pleura on agitation gave off a good deal of air.
33	"	"	" 26	5	Some cruor,—and fibrinous concretion and a good deal of crassamentum in right cavities of heart ; little in left.	The next day the cruor was found coagulated.	Broken up and agitated it gave off no air, and did not apparently absorb any.	Vomicæ, and tubercles in both lungs : left femoral and iliac vein with their branches obstructed by coagulated blood and fibrinous concretions,—the latter in many places softening, and in one or two reduced to a pus-like state.
34	20	Ascites	" 28	27	No blood in heart,—blood from jugular veins liquid.	The following day found liquid.	On agitation it gave off pretty much air.	7 pints of turbid serum in right pleura : 84 ounces of clear serum in cavity of abdomen : some pus in pericardium with great thickening, partial adhesion : a few crude tubercles in lungs, and one small cavity ; the serum from abdomen was of sp. gr. 1018 : the following day, it had deposited a very little lymph : on agitation, it gave off a good deal of air.
35	45	Medullary tumour	Sept. 1	21	A good deal of soft crassamentum, some fibrinous concretions and a very little cruor in heart.	The cruor did not coagulate.	The quantity too small for trial for air.	Masses of cerebriform tumour in left arm, accompanied with much disorganisation of soft parts ; similar disease in substance of left kidney : the lower part of vena cava, the left iliac and femoral veins and their branches obstructed by coagulated blood and fibrinous concretions, the latter in many places softened and puriform.

The instances in the first table were strongly indicative of the variable quality of the blood after death, from disease. The particular instances just given are as strongly corroborative of the same. The variable quality, as far as appreciable by the senses, seems to depend on the lymph, which as regards coagulability, has appeared wonderfully little constant; in some instances retaining its power of coagulation more than 26 hours after death,—in others, coagulating immediately after the fatal event,—and in some, previous to that event. In a large number of the cases, the lymph appears to have existed in the blood possessed of the quality in question in different degrees. This is clearly to be inferred from the results of the *post mortem* inspections, viz. from finding at the same time, and in the same body, in the cavities of the heart, liquid coagulable cruor, soft crassamentum, and firm fibrinous concretions.

The fibrinous concretions, which are so frequently found in the heart after death, are far from being devoid of interest. Their appearance, both as regards form and consistence, is very various. They are also very various in relation to the manner in which they are attached to the heart. I have mentioned one instance in which serum was collected in the interior of a mass; the inner part was soft, the outer surface firm. Some masses seem to have a certain regularity of structure ; some have the appearance of being inflamed, being blood-shot from entangled red particles of blood ; some exhibit the appearance of abscess, and contain as it were in a sac a puriform matter, generated by a peculiar process of softening. Generally they tend to give the idea of the operation of an organising principle,—a *nisus formativus* more or less active until putrefaction commence, by which the animal matter is converted into common matter.

The different varieties of fibrinous concretions are admirably illustrative of certain morbid appearances, especially false membranes, bands of adhesion, the state of the cellular tissue in different kinds of œdema and induration, and, I may add, hepatisation of the lung with and without softening, and a similar state of the spleen, and perhaps tubercles and their softening. When I have examined microscopically the matter yielded by the lung which has been hepatised, undergoing softening, I have found it very similar to the matter into which a fibrinous concretion is converted, whether in the ventricles of the heart or in a vein, composed of particles of different sizes, most of them approaching the spherical in form, and some of them very like pus globules; and I have found the same kind of matter in softened spleen, and also in softened tubercular matter.

No subject, probably, in pathology is more deserving of careful investigation than lymph, in connection with its varieties, supposing either that there are many different species of it, or that it is liable to sudden and great changes of quality. Relative to the former, take for instance the phlebolite in its different stages of induration, from soft lymph becoming of cartilaginous firmness and appearance;

the fibrinous layers deposited in an aneurismal sac, resisting change
occasionally for years; and fibrinous concretions which are formed
in the large veins, especially in phthisis, which so soon soften, and
in part almost liquefy:—how great is the diversity,—how difficult,
in the present state of our knowledge, is the explanation.

Nor is the liquid cruor, or the blood remaining either in part or
altogether liquid after death,—whether for a definite or indefinite
time,—without interest and importance, especially as regards patho-
logical research, and the distinguishing between the effects of dis-
ease and *post mortem* effects in the inspection of bodies. As long
as the blood is liquid after death, it must necessarily observe the
laws to which fluids are subject under similar circumstances; will
accumulate in the dependent parts, and where there is least resist-
ance; and should there be considerable pressure exerted from the
disengagement of air in the stomach and intestines, it will appear as
if injected in the organs removed from such pressure, whether in-
cluded in unyielding parietes, as the brain,—subject to occupy less
volume from greater reduction of temperature after death, than its
solid, and during life cooler case,—or contained in yielding enve-
lopes, as the lungs in the pleuræ and other parts of the body, gene-
rally in the common integuments, and in their peculiar capsules.
In a considerable number of examples given in the table, cruor
was found in the heart more than twenty-four hours after death.
This cruor, in producing false appearances of inflammation and
congestion, would act much in the same manner as liquid blood,
and in estimating the phenomena which present on dissection,
much the same allowance should be made for it,—or serious errors
will be unavoidable.[1]

From the experiments given in the table, on the agitation in air
of the blood collected after death, it would appear that in the greater
number of instances, gas was disengaged. In several instances the
air thus liberated was tried, and was found to be carbonic acid gas,
—proving that there was an excess of this acid in the blood. This
excess, I am disposed to infer, especially from the results of experi-

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[1] Dr. Yellowly in his valuable paper entitled " Observations on the Vascu-
lar appearance in the Stomach, which is frequently mistaken for Inflamma-
tion of that Organ," published in the Transactions of the Medico-Chirurgi-
cal Society for 1813, has forcibly called the attention of medical men to
errors such as those above alluded to. His remarks on the stomach are, I
apprehend, applicable to every other vascular organ; and I am not aware of
any phenomena pointed out by him that are not referable to the circum-
stances specified above. The long time that a portion of blood may remain
liquid in the body after death, does not appear to have been known by him;
indeed, previous to the observations which I have given in proof of it, I be-
lieve it had not been even suspected.

The false appearances of inflammation described by Dr. Yellowly, are not
to be confounded with those, the effects of mere staining: in one instance
the vessels merely contain blood or cruor, as if injected, and which may be
washed out, leaving the part colourless: in the other instance, the colouring
matter is chemically combined with the vessel or texture, and cannot be re-
moved by simple ablution.

ments which I have made on healthy blood, as already mentioned, is the effect of disease, and is particularly apt to take place, as before remarked, in the act of dying, when the powers of secretion seem to be arrested, and carbonic acid probably ceases to be eliminated in the lungs.   In accordance with this, is the uniform dark colour of the blood, which is usually observed after death, and equally in the left and in the right ventricle of the heart; and also the little suffering, fortunately, commonly witnessed in dying,—stupor, or coma, usually preceding and ushering in the fatal event.   From the results referred to, and from a few which I have obtained in operating on blood, taken from persons labouring under disease, I am disposed to think that carbonic acid acts a very important part in the economy of life, and is connected, when in excess, if not with the production of particular diseases, at least with their modification and progress, and the production of certain symptoms,—and that the careful examination of the blood, in relation to this acid, is an inquiry much wanted, and is likely to reward richly those who engage in it.   At present there is a great difference of opinion, derived from experiment, respecting the carbonic acid in the blood : some inquirers, as Dr. Stevens, to whom belongs the merit of having opened this path of research, considering it, in the healthiest condition, always in excess, and capable of being liberated from venous blood, and even from arterial, by the physical agencies in play in respiration; whilst others, as already pointed out, take an opposite view, and deny that any free acid exists in the blood, in its healthy normal state, or can be separated from the blood excepting by processes, in which it is formed from its elements.   This discrepancy, the inquiry I have mentioned will probably explain or reconcile.   It will elucidate also, probably, some other obscure points which might be mentioned, especially where it extended to other fluids, as the urine and bile.   I may mention, incidentally, that whenever I have obtained indications of the disengagement of carbonic acid from the blood after death, they have been afforded by the fluids spoken of, in a very limited number of trials, and also by any effused serous fluid, collected at the same time and subjected to the same test.

In most of the cases brought forward in the second table, the blood was microscopically examined, using the achromatic object-glass of one eighth of an inch focal distance, before mentioned.   I shall notice briefly some of the results.

The blood-corpuscles were seldom found perfectly uniform either in size or appearance; slight differences were perceptible.   In some instances they appeared thinner than usual and more expanded ; in some, of perfectly regular outline ; in others, the discs had an uneven indented margin, and a puckered surface.   In some, in the same specimen, the particles exhibited these variations.   In most cases in which gas was disengaged from the blood on agitation in common air, the blood-discs, under the microscope, appeared to have minute globules attached to them, and very often, instead of gliding on their flat surface, or with a revolving motion, they moved in the current,

passing under the object-glass in a perpendicular position, as if sus-
pended in this manner by particles of air adhering to their upper
edge.

Lastly, in every instance, without exception, in which, during
life, there were purulent discharges, or purulent collections were de-
tected after death, globules, like pus-globules, were visible, under the
microscope, on dilution of the blood with water.

I make mention of these globules in a guarded manner, from the
circumstance that I have occasionally seen similar ones, in instances
in which no suppuration preceded death, and also in the blood of
the sheep in perfect health. Thus, in the month of April, of the
present year, (1839,) on examining, microscopically, the blood of
six different sheep killed the same day,—without exception, parti-
cles resembling pus-globules were detected, using the same method
as that just described. In these, and in the former instances of the
absence of the suppurative process, however, it deserves to be re-
marked, that the particles in question were fewer in number than in
the cases first referred to. Till the exact nature of the pus-like
globules is determined, their pathological influence must necessarily
be involved in some degree of doubt.

I was led to institute these trials, in search of pus in the blood,
from some observations made by Mr. Gulliver, who, I believe, was
the first to detect this substance, or its similitude, in the circula-
tion.[1]

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V.

AN ACCOUNT OF SOME EXPERIMENTS TO ENDEAVOUR TO DETERMINE WHETHER OR NO, FREE CARBONIC ACID EXISTS IN THE URINE OF MAN IN HEALTH.

M. Proust, many years ago, maintained that the human urine
contains carbonic acid ; and M. Vogel, and Mr. Brande, have ex-
pressed themselves to the same effect. The first mentioned inquirer

[1] The observations I refer to, are contained in a very original and ingeni-
ous paper by this gentleman, which was read before the Royal Society last
spring, and which has since been published under the title of "Researches
on Suppuration," in the Philosophical Magazine for September, 1838.

This discovery (supposing that the particles are true pus-globules) which
I attribute to him, of course rests on precise microscopical observation, and
is not to be confounded with the imagined detection of pus in the veins, and
its inferred existence in the blood, in instances designated phlebitis, the ma-
jority of which, hitherto described, I am disposed to believe, were examples
not of true inflammation of vein, but merely of obstruction from the
formation of a fibrinous coagulum, with a tendency to soften and assume a
puriloid character.

was of opinion that this acid exists in excess in the secretion of the kidneys.—" Nos urines (his words are) en sont surchargées."[1]   The last, Mr. Brande, has not adopted this conclusion ; his own is, that the quantity is variable ; he says, " When placed under the receiver of an air-pump during exhaustion, the gas escapes, sometimes copiously, but at other times in minute quantities only."[2]

M. Proust drew his inference from the appearance of the froth which rises in boiling urine, and from the precipitate procured by adding lime to the fluid. His statement on the subject is very brief ; —the following is the whole of it.   " An moment on elles (nos urines) commencent à bouillir, on les voit se couvrir d'une écume blanche volumineuse qui en est gonflée, comme je m'en suis assuré, en recueillant une certaine quantité dans une cloche.   Si, en outre, on y jette de la chaux en poudre, la plus grande partie se change en carbonate."

The experiments which I have instituted myself on this subject, have afforded results which are not reconcilable either with those of M. Proust, or of Mr. Brande.

On the first it appears to me unnecessary to offer any remarks, as the methods employed are of very doubtful accuracy.   In some carefully conducted trials, in which lime-water was added to fresh urine in close vessels, I could detect no carbonate of lime in the precipitate recently examined and not collected on a filter.   If, however, many hours were allowed to elapse, then carbonic acid was extricated from the precipitate on the addition of dilute muriatic acid.

On the effects of the air-pump on the urine, which may be considered an unobjectionable test of the presence of a free gas, or simply absorbed as the carbonic acid in this fluid has been supposed to be, I have made very many experiments, both in Corfu in 1828, and during the last twelve months in this country.   In the Ionian Islands I used an air-pump, in good order but not of great exhausting power.   Recently the instrument I have employed has been of the best construction, the same as that referred to at page 82 of this volume.

In both series of experiments, [taking proper precautions as regards temperature, and the exclusion as much as possible of atmospheric air by receiving the urine, as voided, into vials scrupulously clean, and from which distilled water deprived of air by the air-pump had been the instant before emptied,] the results obtained were on the whole clear and satisfactory.

In the majority of instances, when the exhaustion was carried as far as possible, the urine cooled below 60° remained perfectly tranquil,—indicating the total absence of any gas.

In some comparatively few instances air was disengaged.   To determine the nature of the disengaged air, a little solution of pure

[1] Annales de Chimie et de Physique, tom. xiv. p. 258.
[2] Manual of Chemistry, vol. iii. p. 192, 2nd edition. Mr. Brande, in the last edition of his work, that of 1838, in mentioning the ingredents of heathy urine, includes carbonic acid.

potash was added to the urine, and the exhaustion was repeated. In every example that recourse was had to this test, the disengage- ment of air continued, clearly showing that it was not carbonic acid ; and rendering it probable that it was entangled atmospheric air, introduced in the act of filling the vial,—it being difficult always to prevent the production of froth in this act in a slight degree.

In illustration, I may mention four trials of the same urine, made under circumstances, apparently as nearly alike as possible,—having been received as the bladder was emptied into four vials, just emp- tied of distilled water, and placed immediately to cool, side by side in the open air, at a temperature of about 50°. In about half an hour, they had fallen to about 54°. Then they were in succession placed under the receiver, and the exhaustion effected. No. 1, boiled up very much. No. 2, remained perfectly tranquil. No. 3, gave off a very few particles of air. No. 4, remained perfectly tranquil. In these instances, I did not test No. 1, and 3, by potash, —the other results seeming to demonstrate that the air disengaged was adventitious, and derived from the atmosphere.

The experiments which I have made have been numerous,—on the urine of the same individual at different hours for many days consecutively, and also at intervals at different seasons of the year ; —and further on the urine of different individuals under various circumstances. The general result has been as I have stated, negative, and the more uniformly and perfectly so, the more accu- racy has been observed in manipulating,—the greater the precau- tions taken to prevent the admission of atmospheric air.

From what I have witnessed generally in conducting the inquiry, I am strongly impressed with the conviction, that in a state of health, no free carbonic acid passes from the blood through the kidneys, or is conveyed dissolved in their secretion, and consequently that these organs do not perform a part in the economy similar to that of the lungs, in separating this acid gas from the blood in circulation.

M. Proust, in remarking on the results which he obtained, those already referred to,—seeming to him to show that carbonic acid gas abounds in the urine, says, " we ought not to be surprised at it, as this gas s eliminated from the system in various other ways." " Cet acide, considéré comme ejection ne faut pas nous surprendre. Nous en perdons par la transpiration par l'exhalation pulmonaire, et il fait partie des gaz que la digestion rejette par d'autres voies."

This, fairly considered, does not strengthen his conclusion ; but is perfectly consistent with the opposite one,—that the kidneys do not belong to its regular channel of outlet. If carbonic acid is capable of passing through the kidneys in the state under consideration, it ap- peared to me probable that it might be detected in the urine after drinking largely of a fluid abounding it. I have made two trials with a view to determine this. At Corfu the agreeable effervescing mineral water of Cilli in Styria, which contains an enormous quan- tity of carbonic acid, and some carbonate of lime was drunk fasting,

a pint at a draught; and recently in this country about the same quantity of soda water in the act of effervescing was used. The bladder had been previously emptied. The urine on both occasions, collected within an hour after, was found perfectly tranquil under the exhausted receiver; not a particle of air was disengaged from it, the exhaustion being as complete as possible.

If the carbonic acid, when taken into the stomach in large quantity, is not conveyed into the bladder in the fluid which passes off so rapidly by the kidneys, but is separated from it, either altogether in the primæ viæ, or in part in the blood, it seems highly improbable that these organs, ever, excepting when in a diseased state, should separate this gas from the blood; and when the composition of the blood is taken into account, and the state in which the alkali it contains appears to exist in it,—namely, as I have endeavoured to show, in that of sesquicarbonate,—this conclusion is in a great degree strengthened; and it appears to me also much corroborated by keeping in view the peculiar function and structure of the lungs as an exhaling organ acting exposed to rarefied air, and as it were in a partial vacuum, compared with the kidneys, so differently situated and constituted, and having a function to perform so different.

The principal difficulty I have experienced in coming to the conclusion which I have been obliged to draw from my own experiments has been to account for Mr. Brande's opposite results. I can only reconcile them with those which I have obtained, by supposing that he did not give his special attention to the subject; and that the trials he made of the effect of exhaustion were conducted without those precautions being taken which I have adverted to, and which troublesome experience has taught me are absolutely necessary to guard against fallacy in the inquiry.

I have mentioned as a presumption, that in disease, the human urine may contain carbonic acid. But whether it does in reality, I apprehend remains to be proved. Hitherto, in the trials which I have made of this fluid in patients in hospital, I have not detected it. My experiments on it, however, under the influence of disease, have been limited, not sufficient to allow me to come to any satisfactory conclusion. After death, I have detected it in the urine, the instant it was taken from the bladder; in these cases it may have been generated in the bladder from a change in the urea, which is commonly secreted to the last, and which, as is well known, in its unpurified state, is peculiarly apt to decompose and produce carbonate of ammonia, which, were any other acid present, would necessarily afford carbonic acid gas.

Whether or no azote, or any other gas is ever contained free in the urine, is a question which perhaps cannot at present be answered in a satisfactory manner, and which can be determined only by carefully conducted experiments specially directed to the investigation of the doubtful points.

VI.

NOTICE OF SOME ADDITIONAL EXPERIMENTS WITH THE AIR-PUMP, ON SOME OF THE OTHER SECRETIONS, AND ON SOME OF THE SOLID PARTS OF THE BODY.

1.—*Of Milk.*

I am not aware of any attempts hitherto made to endeavour to determine, whether milk contains free carbonic acid, or that any opinion even has been hazarded on the subject.

Both at Corfu, and recently in this country, I have subjected milk to the air-pump. The results of the best conducted experiments have been negative. At Corfu, in 1828, I made trial of woman's milk, and of cow's milk ; only a very few bubbles of air were extricated on exhaustion ; no more than it might be fairly inferred were entangled in the fluid as it was expressed from the nipple into the receiving vessel. In this country I have experimented only on cow's milk. When precaution was taken to prevent frothing, as by expressing the milk from the nipple, immersed in water deprived of air by the air-pump, in the manner analogous to that used in the similar trials on the blood,—then, on subjecting it to exhaustion when cooled, it remained perfectly tranquil, not a particle of air was disengaged, even when the vacuum was as complete as it could be made.

At Corfu, I made also some experiments on the new laid egg of the common fowl, and with the same results ; neither its white nor its yolk afforded any air *in vacuo.* The trials were made on the white and yolk apart under water.[1]

Are not these negative results in the instance of milk and the egg, such as might be expected, *à priori*, whether we take into consideration their source, or their composition, or the objects for which they are destined ? And, may they not in part tend to account for some of the peculiar qualities in each in their state of perfect newness, and for the superior wholesomeness of the former, when, as by nature it was intended, it is drawn by the infant or young animal direct from the breast of the mother ?[2]

[1] When the whole egg was immersed in water, and subjected to the air-pump, then indeed it gave off some air, as might be expected ; adhering bubbles were detached from the outer surface of the shell generally ; a current of bubbles, for several minutes, was poured out from two or three conspicuous pores in the large end of the egg—air, there is little doubt, which had entered through the same pores, during the cooling of the egg, and its consequent contraction,—which I infer from not finding any air in this part of the egg, when it has been put into water the instant it has been laid, and examined under water.

[2] The air which milk rapidly absorbs, on exposure to the atmosphere, and which is promoted by the frothing in milking, and in the agitation it is com-

2.— Of Bile.

Recently I have subjected, with all possible care, the bile of the ox, the sheep, and the pig, to the air-pump. As soon as the animal was killed, a ligature was applied to the cystic duct, and the gall-bladder was dissected out. In the instance of the ox, the bladder was immersed in water, and brought to me, and the bile, within a quarter of an hour, was received into water that had been deprived of air; in that of the sheep the transfer was made on the spot ; but in that of the pig it was delayed about fourteen hours. In each instance, no air could be extricated by exhaustion; the results were most satisfactory,—not a particle appeared.

On the bile of man, I have recently made only one trial, conducted in the same manner as the preceding ; it was procured from a body, examined twenty hours after death, in the beginning of May, that had died of tubercular phthisis, complicated with paraplegia, occasioned by the growth of a tumour of the medullary kind in the lower part of the spinal canal.   Although a cavity in the lungs was in a gangrenous state, the blood bore no marks of incipient putrefaction ; and not a particle of air was disengaged from the bile *in vacuo.*

In Corfu, I made a similar trial of cystic bile, in two other fatal cases, both of chronic dysentery, with the like result ; one examined 13 hours the other 20 hours after death.

'The deduction from these results is obvious : and, in relation to carbonic acid, as bile is an alkaline fluid, they are such as might be expected *à priori.*

At page 115, I have mentioned, that on trial of bile, collected after death in instances of disease, by agitation with common air, gas was frequently disengaged, which it was supposed was carbonic acid gas.   In these examples it is probable that the disengagement witnessed was owing to incipient decomposition of the fluid, connected with the putrefactive process.

3.— Of synovia and of serous fluids.

Recently, in two instances, I have subjected synovia to the air-pump ; in both, it was taken from the human subject—in one, three hours after death—in the other, sixteen and a half. The trials were made, as already described, by introducing the fluid, as it flowed from the joint, through an appropriate opening, into distilled water purged of air.   In the first example, not a particle of air was disengaged. In the second, two or three very small particles only were separated, giving the idea of adventitious air, entangled during the

monly subjected to afterwards in various ways, conduces powerfully to its fermentation—to the formation of carbonic acid and vinegar. and the separation of its curd and cream.

passage of the fluid from the capsule to the vial; there was not the slightest general effervescence. At Corfu, also, I subjected synovia to the air-pump, in two different instances—the fatal cases referred to in the preceding section—and the results were equally negative.

It has been asserted by a distinguished inquirer, that a small quantity of air is not an uncommon occurrence in the synovial capsules: the words of M. Laennec are—"L'on sait qu'il se trouve souvent, dans les capsules synoviales, une petite quantité d'un fluide aériforme."[1] If so, it might be expected that air might even more frequently be detected in the synovia itself, on the removal of atmospheric pressure; and that the results I have given above are to be viewed as exceptions. But, with a feeling of the highest respect for the individual quoted, I believe that in this instance he has received as matter of fact what is not really so, trusting unduly to loose opinion founded on careless observation. For many years, my attention has been specially directed to the subject of air in the cavities of the body, and I can, without hesitation, say, that in no instance, out of several hundred, in which I have examined the joints after death, could I detect in them the presence of any elastic fluid.

Amongst my notes, bearing the date of Malta, June, 1828, I find three cases, in which careful search was made for air in the capsules of the joints and the sheaths of tendons, by an examination of the parts under water—the bodies having been immersed in a bath for the purpose; one six hours after death, another eleven, and a third twelve. In each instance the result was perfectly negative. As synovia, from its viscid nature, is peculiarly apt to froth or entangle air, it is not surprising that such an opinion as that referred to by the very eminent French pathologist should have arisen.[2]

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[1] Traite de l'Auscultation Médiate, Paris, 1826, tom. ii. 445.

[2] The same opinion is expressed by Laennec in another passage, the correctness of parts of which are very questionable. On account of the importance of the subject, practically as well as theoretically considered, I shall quote his words at length:—"Tous les bruits qui se passent dans l'intérieur du corps, et que l'on peut entendre à l'oreille nue, sont dus aux mouvemens de quelque substance qui se trouve en contact avec un gaz. C'est ainsi que l'on entend les borborygmes dans les intestins, la fluctuation hippocratique dans le pneumathorax avec épanchement liquide, et même celle qui a lieu dans l'estomac, le bruit de la crépitation déterminé par l'inspiration ou par les battemens du cœur dans quelques emphysèmes des parois thoraciques, la craquement des doigts chez certain sujets dont les articulations contiennent habituellement un gaz, un bruit analogue et accompagné de crépitation manifeste sous la main, dans les pneumarthroses qui succèdent fréquemment au rhumatisme articulaire, et particulièrement dans l'articulation du genou."—Op. cit. ii. 456. In this passage, some important truths are mixed with opinions of more or less doubtful accuracy. As the vibrations essential to sound can take place in a fluid, and an elastic fluid is not absolutely required, there is no good reason why several of the sounds mentioned may not be produced without the presence of air; I shall notice only one in particular, by way of illustration, that of the cracking of the fingers—"le craquement des doigts:"—this sound was produced in two of the fatal cases noticed in

Of serous fluids I have hitherto made but few trials; they have been confined to the liquor pericardii and the fluid of the lateral ventricles of the brain. In three different instances (the limit of these trials) in which these fluids, a few hours after death, were subjected to the air-pump, no indications were afforded of the dis-engagement of air, excepting a minute bubble or two, which, pro-bably it is right to infer, arose from air entangled in transferring the fluid from the pericardium and the ventricle into the vessel pre-pared for it. In the instance of the former, it was scooped out with a gallipot, from which distilled water, purged of air, had, the in-stant before, been emptied; in that of the latter, it was allowed to run into a vial nearly full of water, purged of air, through an open-ing in the posterior cornu.

In a case of abdominal dropsy, in which it was necessary to draw off the accumulated fluid by the operation of paracentesis, the trans-parent serum of sp. grav. 1011 was agitated with common air in the double-mouthed bottle already mentioned, without the disen-gagement of any gas; which may be considered equivalent to a negative result with the air-pump.

As the serous fluids are commonly alkaline, [that is, contain an alkali not saturated fully with carbonic acid, and capable of absorb-ing a considerable additional proportion,] the probability is, that in their healthy normal state, they never contain this acid free or un-combined, nor is it likely, reasoning à priori, that they contain any other gas in a state of freedom or mere mechanical solution.

In a state of disease, however, and especially of fatal disease, I believe the presence of carbonic acid in these fluids is not an un-common occurrence. When treating of the blood after death, I have mentioned that I have frequently obtained indications of this gas in serum effused in excess into the great cavities, by agitating a few ounces of it in the double-mouthed bottle, with twice or thrice its volume of common air. In these cases the blood, it should be remembered, was found by the same test to abound more or less in the same acid gas. The source of the carbonic acid, in the in-stances referred to, and whether besides this gas, any other gas is mixed with it, are undetermined points, and seem to be fit subjects for farther and special inquiry.

4.—Of the humours of the eye.

The internal component parts of the eye being from their nature and situation very suitable for the trials under consideration, I have in three different instances subjected them to the experiment of the air-pump. In each instance, the organ was extracted a few hours

the text, in which the capsules of the joints and sheaths of tendons were examined for air, and without a particle being detected, even in the joints which emitted the sound on extension. And this instance, I apprehend, is applicable, by analogy, to all others in which firm or hard surfaces are sub-ject either to attrition or concussion.

after death; a vial having been prepared full of water purged of air, the sclerotic coat was partially divided, and the lens and a portion of the aqueous and of the vitreous humour was received into the water. On one occasion, a small piece of the retina accompanied the lens, and on another, minute portions of the ciliary processes. On exhaustion, in each example the result was similar: no air was disengaged. In the last of the three trials on the eye from different bodies, the contents of one eye were examined three hours after death, and of the other eye the following day, twenty hours later: the result was the same in both. The thermometer during the time, in the room where this body was deposited, was about 58°.

I am not aware that air has ever been observed in the eye during life, or that there is any well-authenticated instance of its having been detected in this organ after death, excepting in cases of very advanced putrefaction.

5.—*Of the solid parts of the body.*

It would be tedious to give in detail an account of the experiments which I have instituted in quest of air in the solid parts of the body. It may suffice to state the general result, or rather the impression made by the results on my mind, which is, that no internal organ, in its healthy normal state, contains air capable of being extricated by the removal of atmospheric pressure by the air-pump, excepting those [the lungs especially] which are designed to be its recipient.

In no instance in which I have tried the substance of the brain, before putrefaction had commenced, has any air been disengaged from it *in vacuo*—a central part having been selected, in which there were no obvious vessels, and carefully cut out with a fine sharp scalpel just dipped in water purged of air, and as soon as cut out, immersed in the same water.

But, in the instances of the other viscera, I have always found more or less air disengaged, on subjecting portions of them in the same manner to the action of the air-pump. The proportion of air evolved appeared to accord with the degree of vascularity of the part. This, I apprehend, is what might be expected *à priori*, taking into account the rapidity with which air enters the arteries, when they are divided, owing to their expansion on the access of air depending on their elasticity. I shall offer a partial proof of an experimental kind.

Two rabbits of the same age were killed: one by dividing the great cervical blood-vessels, the other by a blow on the occiput. The thorax of each was opened before they were cold. In the heart of the first mentioned there was evidently air, and even in the coronary vessels. In the heart of the second there was no appearance of air; it was moderately distended with blood, and its vessels were full of blood. A ligature was applied to the great vessels of each, and the hearts were removed and put into water.

The first swam: the second sank. Under an exhausted receiver, the first was amazingly distended; its blood-vessels also were distended, and their very minute ramifications were beautifully displayed, variously anastomosing: very little of the air was disengaged. The second was not at all distended; a few bubbles of air only were given off, and these came principally from the ends of the divided vessels; the coronary vessels did not exhibit the slightest change, and not a particle of air could be discovered in them.

These experiments were made at Corfu. Recently, I have instituted similar ones (using an air-pump of the best construction) on the carotid artery, containing blood secured by a ligature, and with the like result, as has already been described, when treating of the blood in connection with respiration; and demonstrating, that even if the blood contain carbonic acid gas capable of being extricated by the air-pump, this extrication cannot take place so long as the fluid is contained in the vessels, the pressure of their sides being sufficient, it would appear, to retain the elastic fluid, when present.

Prochaska, in his ingenious treatise on the organism of the human body and its vital processes, whilst he denies, as has been already mentioned, that air is to be found in the arteries after death, yet endeavours to show, that during life, they and other parts of the body partially owe their distention to vapour which, as he supposes, they contain. He says: "Si statum sanguinis in homine vivo et mortuo invicem comparamus, ingens voluminis differentia in utroque adparet; nam qui in vivo homine omnia vasa a maximis ad minima usque repleverat, rubore vitali cutim universam perfuderat, post mortem ex minimis vasis evanescit, cui pallor cutis cadaverosus succedit, venæ subcutanæ collabuntur, et arteriæ a minimis ad maximas usque fere inanes ac vacuæ reperiuntur. Cl. Rosa arteriam vivi animalis sanguine repletam in duobus locis ligaverat, quæ exscissa et refrigerata volumen suum ad $\frac{2}{3}$ diminuit, et sanguis hujus contractæ arteriæ filum referens tantum $\frac{1}{3}$ diametri partem explevit; ex quo sequitur, quod sanguinis volumen in arteria viva ad volumen sanguinis arteriæ mortuæ se ut 9 ad 1 habeat."[1]

The vapour which Prochaska alludes to, it is hardly necessary to observe, can only have effect, and indeed can exist only, where there is free space, as in an artery not full of blood—in the pleura or peritonæum—not at every point in contact with their contents. If such a vapour as he supposes exist in the body, and if the results of Rosa's experiments, quoted by him, are owing to its action, it might be expected to be of a peculiar kind, not merely aqueous vapour, but of some other fluid capable of becoming elastic, and of exerting a distending force at the ordinary temperature of the blood. I shall describe what I have witnessed in repeating the experiments, made with all the care I could use, to endevour to satisfy myself on a point of very great interest, at least, thus hypothetically viewed.

[1] Disquisitio Anat. Phys. Organismi Corp. human. ejusque Processus Vit. Auctore G. Prochaska. Vien. 1812, p. 86.

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1st Experiment.

Laid bare and fully exposed the carotid artery of a sheep, on the 10th May, when the temperature of the air was about 60°; the 8th nerve was separated from the artery. Applied loosely two liga- tures to the exposed part; the superior was first drawn tight; there was no apparent increase of the volume of the vessel below. Im- mediately after tying tightly the lower one, the included space was measured by a compass, and found to be exactly 1·50 inch, and the diameter of the included vessel ·20 inch. The part was now cut out; the artery was first divided above the superior ligature; blood flowed moderately as from a vein from the open extremity; it was next divided below, when the blood gushed out with violence. The portion removed was again immediately measured; its diame- ter was not perceptibly changed; but in length, from 1·50 inch it was contracted to 1·20. It was instantly placed in water purged of air by the air-pump; and about an hour after, placed under a receiver for exhaustion. On exhausting the receiver, I narrowly watched the artery; but could not perceive the slightest distention of it, such as might be expected if it contained any fluid more vola- tile than water; nor did any take place when the vacuum was as complete as it could be made. Measured again, now it was quite cold, and after having been taken out of the water, its diameter appeared unaltered, but its length a little increased, namely, to 1·30 inch.

2d Experiment.

This experiment was conducted precisely in the same manner as the preceding. The portion of carotid artery included between the two ligatures, measured before excision, was 1·40 inch in length, and ·20 in breadth. Immediately after excision the length was 1·32, the breadth remaining unaltered. It was subjected to the air-pump in the same manner and after the same time, and with the same negative result. Now, on admeasurement, no change could be de- tected in its dimensions, either in length or diameter.

In both instances, the artery which was cut out was unusually tense and hard; and when punctured, the contained, and as it would appear, compressed blood, sprang forth with force, and to the distance of about two feet, as if from an animal alive.

These results, carefully considered, I apprehend, can in no wise be received as favourable to Prochaska's views of the existence and operation of an expansive vapour in the living body. As the con- traction of the artery took place immediately on its division and excision, agreeably to what is known of the properties of arteries, and was not increased by cooling, it is not for, but in opposition to, the argument used by the learned professor. The phenomena them- selves, which he endeavours to explain by means of his imagined

cause, do not require it. The heart's action during life seems suffi-
cient to account for the expansion of the vessels, even to the capil-
laries: and its cessation in death, conjoined with atmospheric
pressure, and the elasticity of the arteries, and the force of gravita-
tion, to explain the emptying of the arteries and superficial vessels,
and the consequent pallor, and shrinking of the surface, which is
then witnessed; and as is also witnessed, in a less degree, in mo-
mentary syncope, the same cause, as regards the heart, taking
effect—although unaccompanied by reduction of temperature, such
as would be required to affect the condition of vapour, were it in
existence.

Prochaska makes many excellent remarks on the porosity and
permeability of vessels, admitting of a penetration by moisture,
which, amongst parts in contact, will act the same office as vapour;
indeed, in stating his views on the subject of the porosity of the ves-
sels, he has adduced some curious facts, on an extension of which,
recently, the doctrine has been grounded of endosmosis and exos-
mosis.

Before coming to a conclusion, it may be right to notice an ob-
jection which may be made to the results of my experiments in
search of air in the animal fluids and solids, and the inference
drawn from them, that no air in a state capable of being removed
by exhaustion exits in them. It may be said, that animal mem-
branes are permeable to air, and that therefore they must contain
it, and not they alone, but also the fluids within them. This is
true of the dead membranes—especially the thinnest of them, and
after a time. But it remains to be proved that it is true of mem-
branes generally, and especially during life: and until proved, it
ought not to be admitted. But even were proof of it afforded, it
does not necessarily follow that the pervading air should be found
unaltered within: the oxygen, it is probable, would combine in a
peculiar way to the colouring matter of the blood; and the azote
might also immediately enter into combination. The effect of
porous spongy platinum in effecting the combination of certain
gases is as well known as it is extraordinary; other matters seem
to have an analogous effect: and there is no reason why porous
living parts should not act in the same way.

VII.

ON THE AGE OF MORBID ADHESIONS, AND OF THE FLUID FOUND IN THE PERICARDIUM AFTER DEATH.

It is an opinion pretty generally prevalent, that the age of these
morbid adhesions,[1] which are so frequently met with in the dissec-

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[1] I use the term in its common professional sense, signifying the matter
constituting the morbid connection of parts.

tion of bodies, connecting together serous membranes, may be guessed at by their comparative degree of strength; thus, weak adhesions are usually considered of recent origin—and firm adhesions as being of long standing. Is this opinion correct; and does it agree or not with the properties of coagulable lymph, of which these adhesions are principally formed?

Many circumstances, of which it will be sufficient to mention a few, appear to me decidedly in favour of the negative conclusion.

Wounds, it is well known, which heal by the *first intention*, are often firmly united in twenty-four hours.

In the same space of time, when inflammation has been artificially excited, I have witnessed the formation of strong adhesions. An instance may be given. At Colombo, in the island of Ceylon, in January, 1819, I made the following experiment on a young dog, nearly full-grown. By an opening made with a scalpel between the ribs of the right side of the chest, about a scruple of arrack was injected into the cavity of the pleura. The lung was slightly wounded; air passed freely through the opening, and a little frothy mucus was discharged. The animal seemed at first to suffer pain and to be languid; but left to himself, he gradually recovered, and in the course of the day took some food. At the expiration of twenty-four hours the dog was hot, but apparently not suffering pain; he was running about, and the wound was closed. The dog was now killed, and was almost immediately examined. A good deal of coagulable lymph, which had the appearance of being vascular, was found under the skin round the wound—connecting the cutis and the intercostal muscles. The adjoining cellular membrane was inflamed. Some bloody serum was effused into the right cavity of the chest. Many pretty firm and long adhesions had formed between the pleura pulmonalis and costalis, each of which was of a reddish hue. There were also many adhesions between the pleura and pericardium, and the pericardium itself was inflamed—its surfaces adherent, and, consequently, its cavity obliterated.

The coagulated lymph of the buffy coat of the blood may be used as an illustration of the short time in which strong adhesions may form; and I do not know a better confirmation of the fact. *Liquid*, when the blood is drawn, coagulable lymph gradually becomes—*first, viscid*, and afterwards solid. In the viscid state, as I have frequently observed, when it is still transparent, it has the tenacity of mucus, and admits of being drawn out into fibres and bands, which soon becoming firm and opaque, very well represent the ordinary adhesions of the lungs—and in a very few hours attain their maximum of strength.

This viscidity, which coagulable lymph acquires in passing from a liquid to a solid form, has not, that I am aware, been noticed by authors; and the formation of adhesions is usually explained without reference to this quality.[1]

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[1] Vide "The Morbid Anatomy of some of the most important parts of the

1. It is believed by many writers, and by some of high authority, that the small portions of serous fluid which are found after death in the cavities of serous membranes, especially in the pericardium and the ventricles of the brain, may have been poured out after the cessation of life.[1] I am not aware that this opinion is supported by any precise facts. As a theoretical conclusion its correctness seems doubtful. I have endeavoured to put it to the test of experiment, and the result has been unfavourable. I have notes of three different experiments on dogs, which were made in Ceylon in 1818—all of which seem to show that, under ordinary circumstances, no effusion of serum or exudation, so as to occasion accumulation, takes place after death. The experiments were briefly the following. In each instance a healthy dog was suddenly killed by a blow on the occiput; the cavity of the chest was instantly laid open, and the pericardium inspected; a small quantity of serum found in it was removed by a sponge, and the incisions were then closed by sutures. At the end of twenty-four hours the sutures were divided, and the pericardium was again examined. In no instance had a single drop of fluid collected in the pericardium; and yet, in two instances, the right auricle and ventricle were considerably distended with blood.

If these results be conclusive against fluid being effused into the pericardium, after death, in dogs, the inference from them admits of being extended, by analogy, to other cavities of the same texture, and to man; and I am not acquainted with any pathological observations in opposition to it. The discovery of serous effusions in examinations *post mortem*, no symptoms of their occurrence or existence having been noticed during life, is surely no evidence of their having taken place after the cessation of vital action. It is too well known to be insisted on, that large portions of fluid may accumulate in the pericardium, and even in the ventricles of the brain, without a single symptom indicative of the occurrence.

These remarks were written in 1822, and were published in the Philosophical Transactions for that year. Referring to an able work on physiology, now in progress of publication by Professor Burdach, I perceive that this ingenious inquirer and learned author objects to the results of my experiments, on account of the pericardium having been exposed to the pressure of the atmosphere, which, he is of opinion, would arrest the exudation, "le pericarde ayant été ouvert et exposé à la pression de l'atmosphère, la sécretion ne pouvait plus s'y accomplir."[2] This objection appears to me rather

Human Body," by Matthew Baillie, M.D. F.R.S. fifth edition, p. 6. This reference was made in 1822, in a paper published in the Philosophical Transactions for that year; since that time I do not recollect any writer who has insisted on the above mentioned important property of lymph.

[1] Sauvage's Nosologia Method. *Ephialtes ex Hydrocephalo.* Cours d'Anatomie Medicale. par Antoine Portal, tom. iv. p. 54, 8vo, Paris, 1803.

[2] Traité de Physiologie considerée comme Science d'Observation, par C. F. Burdach, Professeur à l'Université de Kœnigsberg. Traduit de l'Allemande, par A. T. L. Jourdan, tom. viii. p. 509.

specious than well founded. It is true that the air is excluded from
closed sacs, such as the pleuræ and pericardium ; but I do not be-
lieve that the pressure of the atmosphere is prevented, inasmuch
as atmospheric air can enter through the aspera arteria into the
lungs, and through the medium of them, act on the pericardium or
pleuræ—yielding membranes, and ordinarily in contact with the
organs which they contain—and consequently not offering any true
vacuum or an approach to it, favourable to examination.

Reasoning on the subject, and considering the relative qualities of
the parts concerned—the heart, the pericardium, the pleuræ, and
lungs—it appeared to me more probable, that the pericardium would
rather lose fluid than gain it after death by exudation, inasmuch as
the external layer—that dividing it from the pleura—is a thinner
boundary, and it may be supposed, more easily penetrated than the
internal, comprehending with it the muscular parieties of the heart
and its inner lining.

To endeavour to illustrate this view by experiments, I have had
recourse to an aqueous solution of the triple prussiate of potash[1]
(ferro-cyanuret of potassium).

[1] The solution of this salt is peculiarly well fitted for the above purpose,
both on account of the facility with which its presence may be detected in
animal fluids, and its not apparently having any astringent or other effect on
the delicate textures of the body.   The experiments of Hering on the injec-
tion of this salt into the veins of horses may be adduced in proof and illus-
tration ; as may also some experiments which Dr. Wollaston made on the
taking of the salt internally in the year 1798, and some others made by my-
self in 1823.   From Professor Burdach's account of Hering's experiments
given at page 344 of the 8th vol. of his Physiology, it appears that the prus-
siate was often detected in the substance of the kidneys, at the expiration of
a minute after its injection into the vein ; and that it ceased to be detected
in the blood in from fifteen minutes to five hours—although it continued ap-
preciable in the urine very many hours longer, even two days.

Dr. Wollaston, in his paper "On the non-existence of Sugar in the Blood
of Persons labouring under Diabetes Mellitus," published in the Philosophi-
cal Transactions for 1811, has noticed the results of some of his experiments,
when engaged in some similar trials.   He favoured me with a copy of his
original notes, which, as they may interest the physiological reader, I shall
introduce verbatim.   His not being able to detect the salt in the blood, is
explained by the results mentioned as obtained by Hering.   It may be pre-
mised that the figures in the first column indicate the time when the salt was
taken and the urine was tested.

December 15th, 1798.

vii.		pruss. gtt. xxv.=gr. iii.	
vii.	15,	not blue.	
viii.	5,	rather blue.	2.5
	25,	blue.	
	45,	ditto.	
x.	10,	blue.	
xi.	20,	blue.	

Experiment 1.

Two hours after death from pulmonary consumption, the jugular vein of a body was opened—six ounces of blood were allowed to escape, and then twelve ounces of the solution[1] were injected in the direction of the heart, and ligatures immediately applied to the ves-

December 17.

ii.   30, pruss. gtt. xxx. ⎫
iii. 20, not blue.   ⎬ 1.20 shortest time of observed blueness.
iii. 50, blue.   ⎭
iv.   20, ditto.
vi.   30, ditto, strong.

December 26.

vi.   50, pruss. ʒss. ⎫
viii.   ditto.   ⎬ 2.40.
viii. 45, ditto.   ⎭
ix.   30, rather blue.
x.   30, ditto.
      50, V. S. not blue, *i. e.* the serum.

In my experiments a much larger quantity of the salt was taken, viz. 40 grs. of the crystallised salt dissolved in an ounce and a half of water—and as might be expected, the results were more decided. After 15 minutes it was first detected in the urine, and it continued to be detected for the space of 33 hours. The following is a copy of the notes made at the time:—

June 12*th*, 1833.

xii. 40, took the salt.
      42, no effect on urine.
      48,   ditto.
      55, very slight bluish tinge.
      1·5 slight blue.
      15, blue.
v.   0,   ditto.
vii. 0,   ditto.
x.   30, slight blue.

June 13.

viii.   slight blue.
i. P. M.   ditto.
ii.   faint bluish tinge.
x.   very faint.

June 14.

viii. A. M. no perceptible bluish tinge.

I find it remarked, that almost all the urine voided during the 33 hours that the salt was appreciable, was tested—and that the quantity of iron thrown down was very small, conveying the idea that only a small portion of the triple-prussiate reached the bladder—and giving rise to the conjecture that the greater part of it was decomposed in the system.

I may add by way of precaution—that the day after taking the very large dose of the triple-prussiate, I felt unwell—experiencing slight vertigo and tendency to syncope.

[1] 3 grs. of the salt were dissolved in 12 ounces of water.

sel. The *post mortem* examination was made on the following day, the 23d September, twenty-three hours after death.

Not quite half an ounce of fluid was found in the pericardium. On the addition of a little solution of green sulphate of iron, no immediate change of colour was produced; on admixture of a little nitric acid, coagulation was occasioned, and a faint but distinct greenish hue appeared.[1]

There was some crassamentum and cruor, and fibrinous concretion nearly colourless in the right auricle and ventricle—the fibrinous concretions chiefly in the latter. A portion of this fibrin, moistened with the solution of sulphate of iron and a little nitric acid, became blue. In the left ventricle there was a little fibrinous concretion: this tested in the same way, acquired a greenish hue which was only just perceptible. Portions of the lungs were similarly tried. Some portions became greenish; some blue; the blue colour appeared chiefly in the course of the blood-vessels: even the walls of a large tubercular excavation, covered with false membrane, acquired a just perceptible greenish hue.

These results seem to indicate that there was a slight exudation from the heart into the pericardium, but not exclusively so, nor to the extent to occasion the accumulation, which is corroborated by other circumstances.

The serum from the blood, which flowed from the jugular vein after death, (it coagulated and afforded clear serum and well formed crassamentum) was of a bright yellow hue; the liquor pericardii was nearly colourless.

Three drops of the solution of triple-prussiate mixed with half an ounce of serum, tested in the same manner as the liquor pericardii, exhibited as nearly as possible the same degree of colour.

Whence it may be inferred that extremely little of the solution passed into or was retained in the pericardium; that the greater part of it passed into the lungs—and that the liquor pericardii was not a *post mortem* exudation, but a vital secretion.

Experiment 2.

On the 15th September, injected into the jugular of a body, extremely emaciated, which had been dead nineteen hours, nine ounces of a stronger solution of the triple-prussiate;[2] and made the *post mortem* examination two hours after. The circumstances of the case were favourable; there was no serious disease of the lungs, none of the heart; the age of the deceased was 37; the principal lesions were softening of the brain, especially of the fornix, and dropsical effusion into the cavity of the abdomen.

In the pericardium about an ounce of fluid was found: tested by

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[1] Serum prevents the change of colour—the instant coagulation is produced by nitric acid, the colour appears.

[2] 6 grs. of the salt were dissolved in 12 ounces of water.

the salt of iron and nitric acid, it indicated the presence of a very minute quantity of the triple-prussiate; it acquired a very light greenish hue.

There were about three ounces of a similar fluid in the right pleura, and the effect of the test on it was similar: the greenish hue imparted was perhaps a shade lighter. There was very little cruor, crassamentum, or fibrinous concretion in any of the cavities of the heart, and no appearance of any of the solution. A similunar valve of the pulmonary artery tested was strongly coloured bluish-green; the substance of the lungs exhibited on trial the same hue in a less degree, and it appeared even faintly in the frothy fluid contained in the bronchia.

May not these results be considered a confirmation of the preceding, so far at least as regards the wide spread of the solution—and there being no proof of any accumulation of it in the pericardium. It may be said, that accumulation in the pericardium could hardly be expected, as the greater part of it seems to have passed into the lungs. But is it not probable, that when serum separates from the blood in the heart after death, it will pass out through that channel, chiefly, if there is any undue pressure, and that the exudation into the pericardium will be trifling, and even less than from the pericardium into the pleura?

Experiment 3.

On the 18th September, twelve hours after death from complicated organic disease of the lungs, introduced about four ounces of a weak solution of the triple-prussiate into the left pleura, through an opening made by removing a portion of the false ribs; the deceased was 26 years of age. The usual inspection was made three hours and a half after. The pericardium was carefully laid open; it contained about half an ounce of fluid, and by the test before mentioned, it was proved, that a very little of the solution had penetrated into the pericardium and was mixed with it; it acquired a distinct greenish hue. There was a very little fluid in the right pleura; and this on trial afforded proof, by the change of colour, of the saline solution having reached so far.

Experiment 4.

On the 19th of September, an hour and a half after death, the consequence of articular suppuration, after an accident, (the deceased was 45 years of age,) opened carefully into the pericardium, avoiding the pleuræ; and introduced about four ounces of the solution. The opening was closed by suture of the integuments. The *post mortem* examination was made 21 hours after. The left lung contiguous to the pericardium was moist; a few drops only could be collected from its surface. This minute quantity, on the addition

of a drop of the ferruginous solution, became dark-green, and on the addition of a drop of nitric acid, dark blue. The blood in the heart was too thick to admit of being tested.

All these results incontestibly prove a permeability of the membranes after death, admitting of the exudation of fluids. They also seem to show that the amount of exudation is inconsiderable; and to render it highly improbable, that in the instance of the pericardium, pleuræ, or ventricles of the brain, or indeed in the instance of any closed cavity of the body, accumulation (the consequence of exudation,) should take place.

Other facts might be brought forward in support and illustration of the preceding views. I shall very briefly mention a small number.

1. There is no relation between the quantity of blood in the cavities of the heart and the quantity of fluid found in the pericardium. Of this fact, I have satisfied myself by very many observations, taking into account the time elapsing between death and the examination of the body. Occasionally, the one is large and the other small, and *vice versa.*

2. There is no relation, as far as my experience extends, between the quality of the fluid in the pericardium, and the serum of the blood. I have mentioned an instance in which one was colourless, and the other bright yellow. I have witnessed many instances in which the serum of the blood in the heart and great vessels was coloured red, some of the colouring matter of the red particles having been dissolved by it, and in which the same colour was exhibited in the inner lining membrane of the heart and great vessels, the effect of staining, without the liquor pericardii being tinged.

3. Coagulable lymph is very rarely found in the liquor pericardii. But, were the fluid a *post mortem* exudation, it might be expected, that coagulable lymph would be very frequently observed in it, inasmuch as in the majority of instances the blood is slow in coagulating; a portion of it is commonly found liquid in the heart very many hours after death, and which coagulates on exposure.

4. The gall-bladder distended with bile, the urinary-bladder with urine, in fatal cases of apoplexy or palsy, the synovial membranes with synovia, part with their contents with extreme slowness. In no instance have I ever met with synovia in the cellular tissue adjoining the capsule of a joint abounding in synovia. I am not aware of urine having ever been found in appreciable quantity in the cavity of the pelvis after death with distention of bladder.[1] How

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[1] On the 11th of October, 16 ounces of a weak solution of triple-prussiate of potash (strong enough, however, to become dark-blue when mixed with a salt of iron) were injected into the urinary-bladder of a young man two hours after death from pulmonary consumption. The *post mortem* examination was made 24 hours after; the cavity of the abdomen was first opened into and carefully examined; the only fluid found was in the pelvis, between the bladder and the rectum, not quite half an ounce, and, tested by iron, no

small is the exudation from the gall-bladder, and how slow in taking place. Were it not for its colouring quality, the exudation would not be appreciable.

If these views be correct, the question of permeability of membranes and exudation of fluids in consequence, need not greatly embarrass the pathological inquirer. I apprehend, that if no colouring matter of the blood is found mixed with a fluid in a closed serous cavity, it may fairly be inferred, that it was secreted during life; that if the colouring matter of the blood be found intermixed, especially in solution, the inference must be drawn that an exudation has taken place, the amount of which may perhaps be measured with tolerable accuracy by the quantity of colouring matter.

The solution in the serum of the blood of the colouring matter belonging to the red particles is connected with putrid decomposition, and is one of the first indications of it. The permeability of membranes, if not another effect of the same change, is, I am satisfied, greatly increased by it. This is very manifest from the inspection of bodies at different intervals after death, and at different seasons of the year: I believe it may be laid down as a rule that the circumstances favourable to putrefaction, are equally so, to permeability and exudation.[1]

Professor Burdach, when treating on what he considers "secretion after death," adduces the results of the experiments of Ségalas, who, in the examination of animals, found, that if it was delayed to twenty or thirty hours after the death of the animals, the blood was thicker in the right side of the heart, and diminished in quantity by the delay, and that sanguinolent serum had collected in the serous cavities, especially in the pleura.[2]

These results, it appears to me, may be considered analogous to those described in the first experiment which I have related. As in that experiment the greater part of the fluid seems to have passed into the lungs through the natural channel of the pulmonary artery —so in those of Ségalas, the cruor might have had issue in the same way, and if putrefaction had commenced, as seems indicated

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trace of the prussiate could be detected in it; it appeared to be a weak serous fluid—it was rendered turbid by nitric acid.

[1] The difference of effect is well shown in the instance of the urinary bladder. On the 27th of September, when the weather was cool, water, urine, and cruor of blood, were respectively put into sheep's bladders, quite fresh, and suspended in jars, covered from the air. For three hours not a drop of either fluid had passed through; after twenty-four hours, a small quantity of each was found in the receivers, and the serum was coloured red, and even contained red particles. Through a bladder, allowed to become putrid, water passes as through coarse filtering paper. The same effect of putridity, in increasing permeability, is strongly marked in the leather made in warm and in cold climates. In the former, where it is difficult to arrest the putrefactive process, the leather is in a high degree porous, well adapted for use in a high temperature; in the latter, not having suffered from putrefaction, it is dense and almost impervious. It is equally well fitted for use, as a protection from cold and moisture.

[2] Op. cit. vol. viii. p. 509.

by the coloured serum, an exudation might have taken place into the serous cavities, and especially from the lungs into the pleuræ.

VIII.

ON THE SPECIFIC GRAVITY OF DIFFERENT PARTS OF THE HUMAN BODY.

On this subject I was induced to make a set of experiments in 1827, from the belief, that on account of its elementary bearings and probable applications, it was deserving of more extended investigation than it had then received.

Most of the experiments were made in our military hospitals at Corfu, where advantage for carrying on the inquiry was taken of the *post mortem* examination of the bodies of deceased soldiers of our regiments, who died under medical treatment, and whose cases, according to the usage of the service, were recorded.

The parts submitted to trial were weighed almost immediately after they were taken from the body, in every instance in less than six hours, and generally within two. When dissection was necessary, they were carefully dissected out; they were freed as much as possible from contiguous and adhering textures, and especially from fat; and without being previously washed, they were deprived of superfluous adhering fluid, by being gently pressed between folds of blotting paper.

For the sake of brevity I shall give the results in a tabular form: in expressing them, distilled water, the usual standard of comparison, is considered as 1000. The Roman numerals in the second column, indicate the bodies to which the parts subjected to experiment belonged.

Table of specific gravities of different parts of the human body.

No.	Subject.	Part tried.	Result.
1	xi.	Nail of thumb, dissected off entire .	1197
2	ii.	Cuticle of sole of foot .	1190
3	—	Ditto with adhering cutis .	1180
4	xi.	Skin of back of thumb, free from fat, with cuticle undetached	1100
5	iii.	Skin of chest, over the pectoral muscle .	1108
6	—	Ditto with a thin layer of adhering fat .	1050
7	iv.	Skin of epigastric portion of abdomen .	1103
8	—	A front tooth of a young man, not decayed, extracted because loose, owing to disease of the gum .	2240
9	—	Root of the same, the air which was in its cavity expelled	1950
10	—	Crown of the same .	2380

TABLE OF SPECIFIC GRAVITIES—continued.

No.	Subject.	Part tried.	Result.
11	iv.	First molar tooth of a man, about forty years old, extracted on account of pain, connected with a slight caries .	2142
12	—	Roots of the same	2113
13	—	The whole of the upper part of the same above the gum .	2313
14	—	The top of the crown of the same, consisting chiefly of enamel	2620
15	—	A molar tooth not complete, the roots not formed, from the skull of a young woman of Ipsara, which had been exposed about two years to the open air	2500
16	—	A portion of parietal bone from the same skull .	1944
17	ix.	A portion of pars petrosa of temporal bone . . .	1852
18	iv.	Parietal bone freed from pericranium	1772
19	—	Eighth rib freed from periosteum	1383
20	iv.	Fat from beneath the integuments of abdomen[1] . .	0942
21	—	Dura mater	1090
22	v.	Tunica albuginea testis	1088
23	xii.	Sclerotic coat of eye	1091
24	—	Cornea	1076
25	—	Lens	1100
26	v.	Ligamentous sheath of corpora cavernosa penis . .	1097
27	ix.	Aponeurosis covering vastus externus muscle . .	1080
28	vi.	Anterior portion of last lumbar intervertebral substance .	1092
29	xii.	Anterior and outer portion of intervertebral substance, from the lumbar part of spine	1104
30	—	Upper portion of the same, containing some of the articulating cartilage	1110
31	—	The portion beneath, free from articulating cartilage .	1098
32	—	Central parts of the same, soft and pulpy . . .	1062
33	x.	Tendo achillis	1080
34	vi.	Ligament of patella	1104
35	—	Cartilage of knee-joint	1073
36	iii.	Pericardium	1054
37	v.	Cellular part of corpora cavernosa penis . . .	1086
38	iv.	Thoracic aorta	1086
39	vi.	Arch of aorta	1080
40	—	Upper part of femoral artery	1071
41	—	Middle part of the same	1061
42	—	Upper part of the internal iliac artery	1060
43	vii.	Half of a portion of aorta, just below its arch, divided longitudinally	1086
44	—	Other half of the same, its outer fibrous coat detached .	1077
45	—	Lower part of aorta, just before its termination . .	1074
46	vii.	Internal iliac artery	1068
47	v.	Abdominal vena cava	1061
48	xiii.	Vena cava, close to the emulgent vein	1061
49	—	Emulgent vein	1061
50	—	Upper part of femoral vein	1066
51	—	Contiguous vena saphena major	1071
52	vi.	Two of the largest nerves of the femoral fasciculus in the groin	1111

[1] Its specific gravity was ascertained, indirectly, by means of that of a mixture of alcohol and water, in which, when submersed, it remained stationary.

TABLE OF SPECIFIC GRAVITIES—continued.

No.	Subject.	Part tried.	Result.
53	iii.	A portion of cerebrum (medullary and cortical substance intermixed)	1040
54	—	A portion of cerebellum	1043
55	iv.	Pons varolii	1033
56	—	Medulla oblongata	1037
57	i.	Upper part of spinal cord	1035
58	v.	Substance of testicle	1041
59	iii.	Substance of liver	1053
60	iv.	Ditto	1035
61	i.	Ditto	1069
62	v.	Ditto (Lobulus Spigelii)	1066
63	vi.	Ditto	1050
64	vii.	Ditto (Lobulus Spigelii)	1059
65	viii.	Ditto	1042
66	ix.	Ditto	1045
67	iii.	Substance of spleen	1044
68	iv.	Ditto	1064
69	v.	Ditto	1070
70	vi.	Ditto	1048
71	vii.	Ditto	1058
72	viii.	Ditto	1061
73	ix.	Ditto	1060
74	iii.	Substance of pancreas	1047
75	v.	Substance of kidney	1050
76	vi.	Ditto	1050
77	viii.	Supra-renal gland	1048
78	viii.	Thyroid gland	1060
79	v.	Concretion of fibrin from ventricle of heart	1057
80	xiv.	Substance of lung	1054
81	iii.	Hepatised lung	1043
82	vi.	Enlarged lymphatic gland, contiguous to the lower part of abdominal aorta	1046
83	iv.	Muscular substance of left ventricle of heart	1049
84	—	Great pectoral muscle	1058
85	vii.	Sartorius muscle	1056
86	xi.	Lower part of œsophagus	1040
87	—	Cardiac portion of stomach	1048
88	—	Pyloric portion	1052
89	—	Upper part of duodenum	1047
90	—	Upper part of jejunum	1042
91	—	Lower part of ileum	1041
92	iv.	Middle part of ileum	1044
93	xi.	Sigmoid flexure of colon	1042
94	—	Hair	1280
95	—	Ditto	1293
96	—	Ditto	1278
97	—	Ditto	1290
98	—	Ditto	1275
99	—	Ditto	1345
100	—	Ditto	1323
101	—	Ditto	1300

The fatal cases to which the Roman numerals refer were the fol-
lowing.

i.—Aged twenty-seven years; was admitted into hospital on 29th
August; and died 11th September of peritonitis which followed per-
foration of the ileum, the effect of chronic ulceration. The liver
and upper part of the spinal cord appeared to be quite sound. The
dissection was commenced eight hours after death.

ii.—Aged thirty-nine years; was admitted into hospital 29th Oc-
tober; died 31st October, of remittent fever, complicated with bron-
chitis. Dissection eighteen hours after death.

iii.—Aged twenty-eight years; was admitted into hospital 16th
July; died 9th of September of pneumonia. There was much fluid
in the ventricles, and between the membranes of the brain; the sub-
stance of the brain appeared to be sound. The lungs were variously
diseased; hepatised in part, in part dropsical, and tubercular. The
liver exhibited slightly the nutmeg-like section. The spleen was
not enlarged, but rather of a brighter red and harder than natural.
Dissection seven hours after death.

iv.—Aged thirty-four years; was admitted into hospital 9th of
September; died 10th September, of (as it was supposed) remittent
fever. Blood was found extravasated on the surface of the cere-
brum: the trachea and bronchia were redder than natural; the
œsophagus was dark red. The liver was of the colour nearly of
yellow wax; the spleen firmer than natural. Dissection sixteen
hours after death.

v.—Aged twenty-six years; was admitted into hospital 20th Sep-
tember; died 27th September, of (as it was supposed) remittent fever.
The trachea and bronchia were very red; the œsophagus was
highly inflamed. The bile in the gall-bladder was black and offen-
sive to the smell; the gall-bladder itself was in a state of incipient
gangrene. The common gall-duct was pervious; the liver and
spleen were apparently sound. Dissection nineteen hours after
death.

vi.—Aged twenty-two years; was in hospital 116 days; died 2d
October, of chronic dysentery, complicated with tubercular phthisis,
—that is, with tubercles, and vomicæ or cavities in the lungs. The
liver was very voluminous, and rather paler and firmer than natu-
ral. The spleen was large and firm. Dissection nine hours after
death.

vii.—Aged twenty years; was admitted into hospital 30th Sep-
tember; died 5th October, of remittent fever. No organ but the
spleen was particularly diseased. The spleen was of great bulk, of
very soft consistence, like the stale crassamentum of blood; and
after three or four hours' exposure to the air, it became putrid.
From some experiments which I made on it, it appeared to contain
a large proportion of coagulable lymph effused into it, and a very
small proportion of the red particles of the blood.[1] This state of

[1] It probably also contained pus as well as coagulable lymph; in softened
spleens connected with other diseases, I have recently found distinct pus-

spleen, I may remark, almost always presents itself in the fatal cases of remittent fever in the Ionian Islands. The liver was voluminous, soft, and of a brownish grey colour.   Dissection 12 hours after death.

viii.—Aged twenty-five years, was admitted into hospital 18th November ; died 23d November, of (as it was supposed) remittent fever. The lungs, bronchia, trachea, œsophagus, and colon, were more or less inflamed ; the mucous coat of the colon was in places gangrenous.   The liver was rather softer than natural ; the spleen was apparently sound.   Dissection twenty-four hours after death.

ix.—Aged forty-one years ; was admitted into hospital 26th November ; died 27th November, of inflammation of the bronchia and trachea, supervening on latent tubercular phthisis. The spleen was of natural appearance ; the liver exhibited the nutmeg-like section. Dissection eighteen hours after death.

x.—Aged twenty-eight years ; was admitted into hospital 29th September ; died 22d October, of chronic dysentery, after remittent fever.   Dissection nine hours after death.

xi.—Aged thirty-nine years ; was admitted into hospital 5th September ; died 13th November, of latent tubercular phthisis, and other organic disease, after an attack of remittent fever. The œsophagus was redder than natural, and its epithelium in great part abraded. The jejunum and ileum were rather redder than natural, and the inferior part of the latter was slightly ulcerated.   The large intestines generally, with the exception of the sigmoid flexure of the colon, were severely ulcerated.   The spleen was rather smaller and firmer than natural ; the liver presented the nutmeg-like section. Dissection seven hours after death.

xii.—Aged twenty-three years ; was admitted into hospital 29th August ; died 8th December, of tubercular phthisis. The body was extremely emaciated ; the lens was unusually soft, and the intervertebral substance perhaps softer than natural. Dissection twenty-nine hours after death.

xiii.—Aged twenty-seven years ; was admitted into hospital 11th December, died 21st December, of peritonitis, the consequence of perforation of the ileum, the effect of ulceration.   Dissection thirty-one hours after death.

xiv.—Aged twenty-nine years ; admitted into hospital 18th January ; died 7th February, of inflammation of both pleuræ.   A collection of pus was found in the anterior mediastinum ; about three-fourths of a gallon of serum in the left pleura, and about one-fourth of a gallon in the right.   A good deal of coagulable lymph was effused in each cavity, both loose and adhering.   The lungs were very little diseased ; the inferior lobe of the left lung which afforded the specimen, No. 80, the specific gravity of which has been given, was compressed by the effused fluid in a very remarkable manner, and

globules on submitting the pultaceous matter diluted with water to the microscope.

was entirely destitute of air. It bore no marks of inflammation or of hepatisation; and in sulphurous acid, it expanded like the substance of the corpus cavernosum penis.[1]

To these brief notices of fatal cases, it may not be amiss to add some particulars relative to the different specimens of hair of which the specific gravities are given. The first three specimens of hair belonged to English women of the same family, between the ages of thirty and forty years; two were light brown; one was dark brown; all three were fine. The hair, No. 96, belonged to a woman of Corfu, aged sixty-six years; it was gray, and very fine. No. 97 belonged to a man of the same island, aged seventy-seven years; it was white and very fine and soft. No. 98 belonged to an Ipsariot; it was bleached, and probably, in consequence of the destruction of its oleaginous part, its specific gravity was so high.[2] The hair, No. 99, belonged to a Hottentot woman, a native of Cape Town, about twenty years old; it was black, coarse, woolly, and almost felted together, admirably adapted for protection against the sun; but it had not been exposed to its action.[3] No. 100 belonged to a young woman of Pitcairn's Island, in the Pacific Ocean, a daughter of Adam, one of the mutineers of the Bounty; it was reddish brown, partially discoloured by the action of the sun's rays; rather coarse, but not in the slightest degree woolly.

On the results generally I shall offer a few remarks: first, relative

[1] Even when immersed in distilled water deprived of air, and placed under the exhausted receiver of the air-pump, it gave off only a few adhering air-bubbles. It would be difficult to explain this by the mere pressure of the effused fluid. Such a complete absence of air was probably owing to pressure and absorption acting together; and, in confirmation of this opinion, I may mention, that I have found only a very small quantity of air in the lungs of animals which have been killed by strangulation, by the ligature of the trachea.

[2] This hair was taken from the body of an Ipsariot, which when I visited Ipsara, in the winter of 1826, was lying exposed with many others, in the battery, called Fatellio, where they had fallen, making a brave but vain defence against an overwhelming Turkish force, by whom, in the short space of a few hours, this before flourishing island was made a desert, in the most literal sense. Though rather more than two years had elapsed since the massacre, when I saw them, the bodies were little changed; indeed, the features were so distinct, though black and shrunk, that our guide, (the only living inhabitant in an island which before had an active population, exceeding 15,000) recognised each person and called him by name; even their clothes (they had not been stripped) had experienced little change.

The situation of the place where they fell was dry, and its aspect northerly. In the castle of Ipsara, on the contrary, which had a southerly aspect, nothing but whitened bones remained of the numerous victims of despair, who blew themselves up by exploding the magazine, when the place was taken. These facts strongly exemplify the difference of character of the very dry Etesian, or northerly wind of the Mediterranean, and of the very humid Sirocco, or southerly, and especially in the Archipelago.

[3] The effect of the sun's rays on the hair of the negro, is, I believe, analogous to that on the European, &c. but less powerful: I say, *believe*, because I am not quite certain of it.

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to the degree of accuracy which may be claimed for them; and secondly in relation to practical use and application.

As regards accuracy, the results generally can with propriety be considered as approximations only to the truth. The principal sources of error were two—moisture and air, foreign to the composition of the part, both of which it is difficult to remove with that precision which is necessary to insure perfect correctness. The effect of adhering fluid, however, in slight excess or deficiency may, I believe, be held to be of little account in comparison with the other cause, the air, the difference of weight of which, compared with that of solids and liquids, is so enormously great. This source of error exists in different instances in very different degrees, probably very nearly in direct proportion to the vascularity of the part tried, and the size of its vessels, and also to its dryness. This may be deduced from the experiments already related on the action of the air-pump on animal textures. Strictly, in experiments on the specific gravities of bodies, all adhering air, whether superficially attached or contained in the internal structure, should be removed by exhaustion. Whilst I express regret that circumstances did not permit my using this precaution as I could have wished, I shall give a number of examples in which I have since employed it, from which some judgment may be formed of the amount of error attributable to the presence of air.

The first instance I shall offer is that of the parietal bone of a man of middle age, a case of amentia, which proved fatal in consequence of pulmonary consumption.

Before being subjected to the air-pump, its specific gravity appeared to be 1790 to distilled water as 1000; after exhaustion, 1970.[1] The quantity of air disengaged was very great; and it required long-continued pumping to remove it, indeed, after working the pump for more than an hour almost interruptedly, a few particles continued to be disengaged.

A portion of the same bone was calcined. The fixed part, indestructible in the fire, consisting chiefly of phosphate of lime, weighed before being subjected to the air-pump, appeared to be of specific gravity 2580, and after of specific gravity, 3070.[2]

Another portion of the same bone was kept immersed in dilute muriatic acid, until all the calcareous matter was dissolved; the residual cartilage was well washed and afterwards gently pressed between folds of linen to remove all moisture that could then be separated: thus treated, before being subjected to the air-pump, its specific gravity appeared to be 1070, and after exhaustion, 1119.

[1] The bone was dry, prepared for the museum; but it contained some hygrometric water. Exposed to a temperature of about boiling water for three hours, it lost 9.64 per cent. water; making allowance for which, its specific gravity would be, instead of 1970, 2200. This example may serve to illustrate the influence of moisture in ascertaining the specific gravities of animal structures.

[2] The specific gravity of mineral phosphate of lime—apatite, is from 3100 to 3200.

The instances of bone in its dry state, and of its calcareous residue after the action of fire, may be considered perhaps as extreme ones,—in relation to the source of error in question, the hollow texture of both being peculiarly well fitted to retain air. The next examples I shall bring forward, may be considered, perhaps, as rather average ones—namely, the ligamentum nuchæ of the ox, and the middle coat of the arch of the aorta of the ox. The former, before being subjected to the air pump, appeared to be of sp. grav. 1130, and after being so subjected, of sp. grav. 1134 ;—the latter, in the first instance, of sp. grav. 1073, and in the second of sp. grav. 1077.

The substance of the brain may be mentioned as an example of a part not subject to the source of error under consideration, for *in vacuo*, as has been already stated, no air is disengaged from it ; that is, if immersed in water, the instant a portion of it has been cut out a few hours after death,—before the putrefactive process has commenced.

Relative to the practical use and application of the results, I venture to indulge in the hope that the physiologist will receive them as a contribution to his science, considered as a collection of facts and of rules deduced from facts ; and also that the pathologist may derive from them a help to the eye, and a more certain criterion of organic change than we yet possess in this important but obscure branch of medical knowledge ; but for this latter purpose the investigation will require to be prosecuted further. The quality of specific gravity will, probably, be most applicable, as a characteristic to the viscera,—especially the liver and the spleen ; and, perhaps, the brain, lungs, and kidneys. The results given of the experiments on the two first mentioned viscera are favourable to this conjecture. These results are very various, and almost demonstrative that organic disease of the viscera cannot take place without altering their specific gravity ; and, if so, it is not improbable that this precise quality may be made an index of the kind and degree of organic change. And, *à priori*, perhaps, thus much might be inferred ; for if fatty matter is deposited, the specific gravity must be diminished ; if cartilaginous matter, it is probable it will be increased ; probably it will be variously diminished or increased according to the kind of deposition. Moreover, attention to these changes may lead to more minute observation, and to the collecting of information curious and useful. Every new means of research is in this way indirectly beneficial. Whoever employs the stethoscope, though he may not appreciate its merits in the same high degree as the ingenious inventor of the instrument did, yet he cannot fail to derive instruction from its use, and to become better, more minutely, more discriminately acquainted with the diseases of the chest. The same remark applies to Avenbrugger's method of diagnosis by percussion, and indeed to every other method of investigation, which directs and fixes the attention,—drawing it away

from vague and unmeaning generalities, the bane of knowledge, to precise and significant particulars,—which are its essence,—and which are equally important, whether we have in view, during life, the symptoms of disease by the bed-side of the patient ; or, after death, on the dissecting table, the organic changes, the effects of diseased action.

The greater portion of the preceding observations, first appeared in print in the 3d volume of the Edinburgh Medico-Chirurgical Transactions for 1828. Recently, on consulting Professor Burdach's Treatise on Physiology, I find that the subject under consideration has, since that time, been investigated by MM. Schubler and Kapff, who published a work on it, in 1832. Comparing their table of specific gravities of the different textures of the human body, contained in the 8th volume of the treatise on physiology already referred to,—with mine,—some of the results are found to agree, others to disagree, as perhaps might be expected, partly from real differences of specific gravity in the same textures in different bodies, which is strongly exemplified in my list, and partly from the methods of operating and the interference of minute and obscure causes of disagreement and error, not easily detected, independent of the two main ones already pointed out. The most striking difference between their results and mine, presents itself in the instance of the lungs: and I select it the rather for comment, as it affords illustration of one of the causes of difference. According to MM. Schubler and Kapff, the specific gravity of the substance of the lung is 645-to water as 1000 ;[1] according to the result obtained by me, 1054. I have stated that the specimen which I subjected to experiment was destitute of air : their specimen, of course, contained air : and it follows that as the air in the lung is in variable quantity, its specific gravity also, unless air be excluded, must be variable also.

IX.

ON THE EFFECTS OF DESICCATION. AND THE PROPORTION OF WATER CONTAINED IN THE DIFFERENT TEXTURES OF THE HUMAN BODY.

This subject, like that of the specific gravity of the different textures, may be considered also as elementary and fundamental ; for unless it is known with tolerable accuracy what is the quantity of dry solid matter, each part is capable of affording, or may be said to contain, other statical results will of necessity be vague and of little value.

[1] Traite de Physiologie, tom. viii. p. 6.

It was in Malta, in the summer of 1828, that I commenced the inquiry, desirous of conducting it on a more extended plan than I believe had been previously attempted, by any one individual; so that, one method being employed, and the circumstances being similar, the results might be compared with confidence, and discrepancies avoided.

The method of desiccation which I employed was simply that of the vapour-bath. The parts were either placed on paper when not likely to adhere, in a Wedgewood evaporating dish, over steam; or suspended over it almost touching the bottom, by a fine thread, when of an adhesive kind; or, when very soft, they were placed in it, in platina capsules previously weighed. They were kept on the bath till all the water capable of being expelled by a temperature of 212,° was driven off, which was indicated by their ceasing to lose weight from further exposure.

The results, first published in the 4th volume of the Transactions of the Medico-Chirurgical Society of Edinburgh, are contained in the following table, constructed on the same plan as the preceding, in which the Roman numerals are designed for similar reference.

Table showing the proportion of dry solid matter in different parts of the human body.

No.	Subject.	Part tried.	Result per ct.
1	i.	Cuticle of sole of foot	51.5
2	iii.	Projecting part of the nail of thumb, rather curved as in phthisis	83.3
3	—	Inner part, soft, diaphonous, carefully freed from adhering subjacent layer	76.6
4	—	Bosjesman's hair	91.6
5	—	Hottentot woman's hair	92.0
6	—	Kaffer boy's hair	92.3
7	—	Young woman's hair of Pitcairn Island . . .	91.3
8	—	Light coloured hair of an Englishwoman . .	90.7
9	—	Dark coloured ditto	90.7
10	ii.	Skin over belly of deltoid muscle . . .	32.4
11	iv.	Ditto over lower part of scapula . . .	35.3
12	v.	Ditto from middle of back	42.3
13	vi.	Ditto　　Ditto	39.5
14	vii.	Ditto over first joint of thumb . . .	39.0
15	—	Ditto of fore-arm just below its bend . .	32.8
16	—	Ditto of shoulder	37.4
17	—	Ditto of scrotum	30.2
18	vi.	Cornea	27.3
19	—	Sclerotica	33.3
20	i.	Fatty matter from beneath integuments of the abdomen	88.9
21	—	Pectoralis major muscle	15.6
22	iii.	Muscular substance of left ventricle of heart .	20.5
23	—	Pectoralis major muscle	24.0
24	v.	Gluteus maximus muscle	25.8
25	—	Columna carnea from left ventricle . .	21.2
26	iii.	Cerebrum (centrum ovale). . . .	33.9
27	—	Cerebellum	20.8

TABLE OF SPECIFIC GRAVITIES—continued.

No.	Subject.	Part tried.	Result per ct.
28	iii.	Medulla oblongata	31.0
29	v.	Sciatic nerve	28.5
30	i.	Aorta (inferior abdominal portion)	22.5
31	iii.	Ditto at its origin	28.0
32	v.	Ditto at its origin	34.5
33	—	Femoral artery just below the arteria profunda	31.4
34	vii.	Ditto Ditto	34.4
35	—	Aorta just below its arch	31.7
36	—	Ditto just below the cœliac	28.2
37	—	Ditto (its termination)	26.5
38	—	Common iliac artery	24.6
39	ix.	Basilar artery	26.8
40	—	Arteria sylvana	31.0
41	x.	Basilar artery	34.0
42	—	Arteria sylvana	31.4
43	i.	Ascending vena cava.	27.9
44	v.	Femoral vein contiguous to arteria profunda	31.3
45	vii.	Ditto a little lower	32.3
46	—	Vena cava ascendens (its commencement)	25.0
47	—	Superficial vein at the bend of the fore-arm	33.3
48	vi.	Thoracic duct	29.5
49	i.	Substance of liver	22.9
50	iii.	Ditto	24.3
51	v.	Ditto (Lobulus Spigelii)	25.8
52	viii.	Ditto	28.5
53	i.	Substance of spleen	19.2
54	iii.	Ditto	24.3
55	i.	Substance of kidney	19.0
56	iii.	Ditto	18.4
57	i.	Substance of lung	22.2
58	iii.	Substance of pancreas	22.2
59	iv.	Substance of testicle	16.3
60	—	Great arch of stomach	19.5
61	—	Upper part of jejunum	18.6
62	viii.	Ditto	11.8
63	iii.	Dura mater	32.7
64	i.	Pericardium (outer layer and a little fatty matter under it detached)	41.0
65	i.	Perichondrium of cartilage of false rib	35.0
66	iii.	Ditto	35.0
67	vi.	Pleura costalis	31.0
68	ii.	Corpus cavernosum penis	22.2
69	iii.	Extensor tendon of great toe	32.2
70	v.	Tendo Achillis	32.0
71	iv.	Intervertebral substance from last lumbar intervertebral space, cut out in the form of a wedge	26.7
72	v.	Cartilage of knee-joint	28.0
73	ii.	Cartilage of false rib	33.7
74	iii.	Ditto	40.0
75	i.	Superior part of sternum (spongy portion)	57.3
76	—	Ditto (outer table)	78.3
77	v.	Shaft of tibia (outer layer)	89.2
78	—	Ditto (inner layer)	88.0
79	iii.	Fibrinous concretion from right side of heart	23.0
80	viii.	Ditto from left auricle	17.5
81	—	Venous blood, of specific gravity 1061	20.7
82	—	Serum of blood, of ditto ditto 1030	11.7

The parts submitted to experiment were taken from the following subjects.

i.—Aged twenty-nine years; admitted into hospital 20th May; died 2d June, of inflammation of the mucous coat of the ileum and colon, ending in gangrene. Dissection seven hours and a half after death; body slightly emaciated.

ii.—Aged thirty years; admitted into hospital 23d May; died 30th May of tetanus, following a fracture of the inferior extremity of the radius, and of two transverse processes of the lumbar verte-bræ. Dissection sixteen hours after death; body very muscular and fat.

iii.—Aged thirty-six years; admitted into hospital 3d May; died 5th June, suddenly, with tubercular phthisis. Dissection three hours after death; body considerably emaciated.

iv.—Aged twenty-two years; admitted into hospital 27th April; died 9th June, of tubercular phthisis, very far advanced and com-plicated. Dissection seven hours after death; body extremely ema-ciated.

v.—Aged twenty-three years; admitted into hospital 20th May; died 13th June, from fracture of the cranium and the cervical ver-tebræ, with effusion of blood on the brain, and softening of the spinal chord. Dissection twelve hours after death; body not ema-ciated.

vi.—Aged twenty-two years; admitted into hospital 1st July; died 4th July, suddenly, as if from syncope, whilst labouring under a frebrile attack, connected with ulceration of the mucous mem-brane of the ileum, and enlargement and softening of the mesen-teric glands. Dissection eleven hours after death; body not ema-ciated.

vii.—Aged thirty-two years; admitted into hospital 5th July; died 13th July, with febrile symptoms, connected with a vast latent abscess in the liver. Dissection sixteen hours after death; body not emaciated.

viii.—Aged thirty-three years; admitted into hospital 3d July; died 16th July, of acute dysentery, terminating in gangrene of the mucous coat of the large intestines, with perforation of the colon. Dissection seventeen hours after death; body slightly emaciated.

ix.—Aged thirty-two years; very stout and muscular; died sud-denly in barracks, of pulmonary and cerebral ecchymosis. Dissec-tion fourteen hours after death.

x.—Aged twenty-three years; admitted into hospital 17th July; died 30th July, of inflamed and ulcerated ileum. Dissection twelve hours after death; body considerably emaciated.

With the exception of the parts taken from the body No. 1, all the others, the instant they were dissected out, were gently pressed between folds of blotting-paper, to remove superfluous mois-ture or adhering fluids; and in an hour, or two at farthest, were placed to dry on a vapour-bath, as already mentioned. In the

exception above alluded to, the parts were kept together, covered over, till the following morning, and then they were washed with water, and subjected to pressure with blotting-paper, previous to drying.

It may be right to state, that all the bodies were of British soldiers, and that they all died in Malta; and, as will appear from the dates, during the hot season; the thermometer during the whole time fluctuated between 80° and 90°.

Of the effects of desiccation on animal textures, remark is hardly required, as they are pretty generally known, and are such as might be expected à priori. The brittleness which the operation imparts, and its preventing putrefaction, are two of its best defined and most general effects. But these effects are only temporary; as soon as humidity is restored, flexibility is also restored, and the tendency to putrefaction and decomposition returns, and apparently unimpaired by suspension. This property of desiccation in animal matter, and in vegetables also, though it has been long applied to economical purposes, for the preservation of articles of food, has not, perhaps, been used so much as it deserves; nor has the principle, I believe, been applied as generally as it might be, to the solution of certain phenomena, strictly chemical, which occur in nature and art. Does it not serve to explain a fact which, on first consideration, seems not a little extraordinary, that meat will remain fresh for many days in Egypt, if hung up in the open air, and exposed to sunshine and wind, in the hottest weather; whilst it putrefies in a few hours if confined in a close damp place? Does it not afford equally an explanation of the opposite effects in the Mediterranean, on dead animal matter, of the southeast or sirocco wind, and of the north and northeast winds, of which a remarkable example has already been given, witnessed in Ipsara? The former wind, almost saturated with moisture, produces little or no evaporation, or desiccating effect; whilst the latter, deficient in moisture, and sometimes in an extraordinary degree deficient, occasion rapid evaporation, and powerfully desiccate; the one acting like confined moist air of high temperature, and greatly promoting putrefaction; the other operating like the dry wind of the desert, though in a less degree, and retarding the change.[1]   May it not, too, serve to account for so many remains of ancient Egypt, especially of Upper Egypt and of Nubia, being preserved to the present times, the natural dryness of the climate having been assisted by various processes of art?   And is not proof afforded of the correctness of the explanation, in this region being almost the only one thus spared and ex-

[1] The cooling effect of the wind is one of the simplest and most certain means of knowing its proportional humidity. The cooling effect on a thermometer, the bulb of which is wrapped in moist muslin, of the sirocco wind, varies from 3° to 6°, and of the northerly winds in summer from 12° to 30°. This is the result of my observations in the Ionian Islands and Malta, and it is applicable, I believe, to the winds named, in the Mediterranean generally.

empted from the general doom of slow and certain change, almost amounting to destruction, which is manifest in other countries, where the elements have free play through the intervention of moisture, even in the works of nature?

Looking over the preceding table of results, it is curious to notice what little relation there is between the solidity of a part, indicated by firmness and resistance, and the quantity of solid matter which it yields on desiccation. Who would suppose, *à priori*, that the soft flexible skin contains more of this solid matter than cartilage; or that the delicate serous membranes contain more than the firm viscera; or (and it is most remarkable) that the liquid blood possesses a larger proportion of it than some of the solids?

The proportional quantity of solid and fluid matter constituting the animal body, is a question to which considerable attention has been paid by physiologists; and it is a problem still unsolved. According to some, the proportion of the former to the latter is as one to six; and according to others, as one to nine. The results of the experiments on desiccation which I have given, including those on blood and serum, may help to furnish accurate notions on the subject;—to show in the first place, how extremely difficult it is to solve the problem rigorously, and how impossible it is to solve it generally; and, in the next place, to show that, in the adult at least, the proportion of fluid or of water has been overrated, inasmuch as in the blood itself it does not exceed one fifth.

As my experiments were commenced unconnected with any speculative views, so I shall endeavour to abstain as much as possible from such views, as they are very apt to lead astray, and in no inquiries more than in physiological ones, and pervert the judgment, and even the senses. Deductions must be drawn from experiments with great care and hesitation, and not from one or two, but from many results. The conclusions which may obviously be made from the preceding experiments, it is unnecessary to point out in detail; and any others I would rather abstain from drawing, excepting conjecturally, and for the purpose of leading to further inquiry. Thus, in the instance of the results of the experiments on the arteries and veins, it may be asked, do they not indicate that the proportion of solid matter in the former decreases with the distance from the heart, excepting in the brain, where the arteries are very analogous to veins;—whilst, in the latter, would it not appear, that it observes a contrary ratio, and increases with the distance? Should ulterior investigation confirm this, it will be an interesting fact established in physiology.

X.

ON THE ACTION OF CORROSIVE SUBLIMATE ON THE TEXTURES OF THE HUMAN BODY.

The power which corrosive sublimate possesses, of preserving animal substances, has been known for many years, and has been taken advantage of by anatomists, both in the dissecting-room and the museum, to promote the objects of each, as has been well demonstrated by the minute and elaborate labours of Mr. Swan on the Nervous System. The property also which it has of precipitating albumen from animal fluids has been known for a considerable time, and, as a valuable test in analytical chemistry, has been studied with some care. But I am not acquainted with any researches hitherto made to investigate its action on the textures of the body generally. Conceiving that such inquiry was likely to afford results which might be interesting and useful, I applied myself to it in Malta, in 1828. I shall describe such of the experiments which I then made as I believe are to be depended on; with the hope that they may lead to a renewal of an investigation which unavoidable circumstances obliged me to leave in a very unfinished state.

The two first I shall mention were of a preliminary kind, to test its power of arresting fermentation and putrefaction.

On the 15th August, at a temperature between 80° and 90°, a small quantity of aqueous solution of corrosive sublimate was added to the expressed juice of the grape in brisk fermentation. The fermentation was immediately arrested. The mixture was put by in a bottle, loosely corked, not entirely excluding the air. Examined on the 24th February, no further change appeared to have taken place; the liquid was clear, and had the peculiar smell of sweet must, and the sediment seemed unaltered.

On the 10th of August, a portion of muscle putrefying was put into a solution of corrosive sublimate. The putrid process was arrested; the peculiar offensive odour ceased. It was then washed and suspended in water. Examined on the 24th February, it was found unaltered—totally free from any unpleasant smell.

On the 15th of April, portions of different textures, taken from the body of a soldier that had died at the age of 36, were immersed in a saturated aqueous solution of corrosive sublimate, after having been gently pressed between folds of filtering paper, and carefully weighed. They were left undisturbed till the 25th May, when they were taken out, examined, and weighed: they were then replaced. On the 6th July, they were again examined and weighed, first, in a moist state, after being pressed between filtering paper; and afterwards dry, having been thoroughly dried on a vapour bath.

The following table exhibits the results; the column under 15th
April gives the weight of the portions tried before immersion; and
the two following, the weights of the same, after continued immer-
sion.

No.	Texture tried.	April 15.	May 25.	July 6.	Dried.	Dry matter per ct.
		grs.	grs.	grs.	grs.	grs.
1	Costal cartilage	17·8	18·6	18·7	7·7	43·2
2	Intervertebral substance	19·6	19·6	19.2	5·4	27·5
3	Rib	17·2	14·2	13·2	7·9	46·2
4	Upper part of spinal cord	14	14.4	13·5	5·3	37·8
5	Pectoralis major muscle	45	26	27	12·6	28
6	Liver	23	20·4	19·4	8·4	36·5
7	Spleen .	26	18·5	17	7	27
8	Dura mater	9·8	8·9	8	3·05	31
9	Pia mater	4·2	3.4	3·1	1·5	35·7
10	Lung	19·8	12·5	11·6	4·6	23·2
11	Cellular structure	2·9	2·4	2.4	1·3	44·8
12	Fatty matter	6·6	6·3	6·6	5·8	88
13	Kidney .	40	30	24.3	6·9	17·2
14	Pancreas	25	15·2	14·7	6·1	24·4
15	Fibrinous concretion	10·3	8	7·4	3·7	36
16	Vena cava .	14·2	11·5	10·6	3·9	27·4
17	Aorta .	10·1	8·2	8·1	3·2	31·6
18	Corpus cavernosa penis	15·8	9	—	3·3	20·9
19	Cuticle of sole of foot .	6·4	16·6	—	4.3	67
20	Cutis vera .	11·9	10·5	—	4·3	36·1

It is noted down at the time, that when the different parts above
mentioned were examined on the 25th of May, they were all com-
paratively pale, appeared to be condensed, and emitted no unplea-
sant smell, and were perfectly distinguishable: a little calomel had
formed on the sides of the glass vessel in which the experiments
had been made. And, also, that when examined on the 6th July,
they appeared to be exactly in the same state. The solution itself
seemed to be but little altered; it contained only a very trifling
sediment. Evaporated to dryness, it yielded a fawn-coloured resi-
due, which, strongly heated, first gave off fumes of corrosive subli-
mate; next, whiter and denser fumes, as if of calomel; and, lastly,
an ammoniacal vapour: a small quantity of carbonaceous matter
remaining.

On examining the subjects of the experiments, after they had
been dried, with a powerful lens, minute crystals of corrosive subli-
mate were perceived in several of them, principally in those the
weight of which was most increased. To separate the loosely ad-
hering salt, they were immersed and left in water, form the 8th to
the 15th July, during which time the water was frequently changed.
They were then again weighed, after thorough drying on a vapuor
bath. The following table exhibits the results:—

No.	Texture tried.	Dried.	Dried matter per cent.
1	Costal cartilage - - -	7·2	40·45
2	Intervertebral substance - -	5·1	26
3	Rib - - - - -	7·5	43·8
4	Upper part of spinal cord - -	4·8	34·3
5	Pectoralis major - -	11·3	25·1
6	Liver - - - -	7·9	34.3
7	Spleen - - - -	6·4	24·6
8	Dura Mater - - -	2·85	29
9	Pia Mater - - - -	1·45	34·5
10	Lung - - - -	4·2	21·2
11	Cellular structure - - -	1·3	45
12	Fatty matter - - -	4·5	68·2
13	Kidney - - - -	6·2	15·5
14	Pancreas - - - -	5·8	23·2
15	Fibrinous concretion - -	3·1	30·6
16	Vena Cava - - -	3·5	24·6
17	Aorta - - - -	3	29·7
18	Corpus cavernosum penis - -	3·1	20
19	Cuticle of sole of foot - -	3·6	56·2
20	Cutis vera - - - -	4·1	34·4

It may be remarked, that none of the textures during this second immersion in water, showed the slightest signs of putridity, or tendency to decay; this indeed might have been expected, considering the result of severer trials. The white parts generally, especially the cutis, blood-vessels, and membranes, absorbed water, and became more or less flexible, well adapted to display their form and texture in a class-room. The portions of lung, kidney, and liver remained firm; the pancreas and muscle were softened and rendered flexible in an intermediate degree.

Comparing the results contained in the last table with those in a former table, showing the effects of desiccation, it seems very doubtful if the corrosive sublimate combines with the textures generally, or with any one texture entirely. Probably it unites only with a part of each, that part on which putrefaction chiefly depends, and which, it may be conjectured, is essentially albuminous, and not very different in its nature from the albumen ovi, for which the chloride has a strong affinity. The proportionally increased weight of the fibrinous concretion, of the liver, and muscle, is in favour of this conclusion.

It may be asked, does corrosive sublimate combine directly with albumen—as from the manner of expressing my opinion in the last paragraph, seems to be taken for granted? My belief is, that it does so combine. This was the original view of Dr. Bostock, as stated in his ingenious paper on animal fluids, published in Nicholson's Journal for June, 1806, in which it is also taken for granted, as most natural, and in accordance with facts. It appears to me preferable to the later conclusion of M. Orfila, that the compound precipitated from white of egg by corrosive sublimate, consists of calo-

niel and albumen; and preferable also to the more recent conclusion of Rose, that it is composed of albumen and oxide of mercury. Orfila drew his inference from the products of the destructive distillation of the compound[1]—results ill adapted for forming an opinion on the subject.   Moreover, at that time, viz. in 1814, the old hypothesis respecting the compound nature of chlorine still prevailed in Paris.   The data on which Rose reasoned I am not acquainted with; I have not seen his original paper—merely a reference to it in the edition of Dr. Turner's Elements of Chemistry for 1834.

The facts which induce me to adhere to the early opinion of Dr. Bostock, are chiefly these:—

1. When corrosive sublimate and albumen ovi combine, there is no disengagement or production of muriatic acid, as might be expected—whether it contained calomel or oxide of mercury.   In one experiment, in which the whites of two eggs were mixed with a drachm of corrosive sublimate, and after coagulation at a temperature of about 120°, washed with a small quantity of water, and filtered, the water which passed through the filter had no well marked acid property, and did not occasion any effervescence when mixed with a solution of carbonate of potash.

2dly, I have not been able to combine calomel and albumen. When I have triturated together the white of egg and calomel, the latter has become gray, almost black, as if from the formation of a little protoxide of mercury from the action of the alkali contained in the white of egg: but the white of egg itself has undergone no change.

3dly, and lastly, The attempts I have made to combine albumen and oxides of mercury directly, both the protoxide and peroxide have been equally unsuccessful.

The properties, too, of the compound appear to me most favourable to the idea which I have advocated of its nature.   Its properties are strongly marked, and are indicative of energetic union—being tasteless, insoluble in water, and not decomposable either by the muriatic acid, or by an alkali, or by lime; whilst it is soluble in an aqueous solution of the former, whether of soda, potash, or of ammonia, and also in a slight degree in the liquid albumen ovi.

Dr. Bostock, from his experiments, concluded that the compound of albumen and corrosive sublimate contains from $\frac{1}{3}$ to $\frac{1}{4}$th its weight of the latter.   From various circumstances, it is difficult to ascertain the exact proportions of the two, as he himself has pointed out. The estimate can only be considered as an approximation: I mention it chiefly in application to the preceding results.

These results, practically considered, are not without interest, as confirming the general antiseptic power of corrosive sublimate— preventing, it would appear, putrefaction from taking place in every instance in which it was tried—arresting it when in progress, and equally arresting vinous fermentation.

[1] Traité des Poisons, tom. i. part 1, p. 47.

Mr. Swan, in his "Account of a new method of making dried Anatomical Preparations," published in 1820—which I have only recently seen—has shown how usefully corrosive sublimate may be employed in the dissecting-room; in consequence of its great antiseptic power for the purpose of carrying on minute dissections, and for making dried preparations. He employed it in solution in spirit of wine, (2 oz. of the chloride to 16 oz. of spirit of wine,) or in water with the addition of muriate of ammonia (2 oz. of the chloride, 70 grains of the muriate, and a pint of water). The latter is recommended by its cheapness. For the details of Mr. Swan's method, I must refer to his work on the subject, which is very deserving of the notice of the practical anatomist.

Corrosive sublimate in solution has also been employed for preserving moist preparations; but hitherto not successfully. In 1823, I saw several preparations preserved by means of it, in the museum of the late Mr. Brooks, and in the collection of Mr. Carpue. Mr. Brooks informed me, that after steeping for a certain time the preparation in an aqueous solution of the chloride, he transferred it to proof-spirit, covering the jar in the usual manner. All the preparations which he showed were more or less injured by a white deposite encrusting them: and in some instances, the insides of the jars were so encrusted as to have become quite opaque, hiding completely the object within. From some experiments which I then made on two preparations which had there suffered, which he was so good as to entrust to me for examination, I found the encrusting matter to be chiefly calomel—whether on the preparation itself, or on the interior of the containing vessel: the latter deposit was in one instance mixed with a little fat and other animal matter.

From a set of experiments which I made on corrosive sublimate in 1822, and which were published in the Philosophical Transactions for that year, I showed, that under certain circumstances of complicated affinity, corrosive sublimate is very apt to be decomposed by exposure to light, and calomel precipitated; and that the effect may be prevented by adding a substance which has a strong affinity for the chloride, such as muriatic acid, or muriate of ammonia, in a saturated solution of which it was, I found, seventeen times more soluble than in water alone.

Such complicated affinities, I have no doubt, were exercised in occasioning the precipitation of calomel, in the instances of the spoilt preparations to which I have alluded: and it is highly probable that the bad effects might have been prevented by certain precautions, tending to oppose those affinities, as by immersing them after steeping in the chloride solution in dilute muriatic acid, or in a solution of muriate of ammonia; or placing them immediately in a compound solution of the muriate and chloride. The few experiments I have made of this mixture have given favourable results; but they have not been carried sufficiently far, or made with sufficient precision, to enable me to give a decided opinion on its *permanent* efficacy. This I regret, considering the cheapness

of the means, and the promise of success which the method holds
out. By *permanent*, I mean not a duration of a few months, or
even of a few years, but an unlimited time—in relation to use for
the preservation of valuable preparations in the museum. To de-
termine this, trials on things of little value ought to be made, with
a careful record of dates and effects. With this design, a series of
experimental preparations have recently been put up in the patho-
logical collection at Fort Pitt, and will, I hope, hereafter afford data
for arriving at the information required.

XI.

OF THE ACTION OF LIME ON THE TEXTURES OF THE HUMAN BODY.

It is commonly asserted and believed, that lime exercises a cor-
roding destructive influence on animal matter in general—and that
animal bodies, exposed to its action, rapidly decompose and disap-
pear. Accordingly, it has been almost invariably recommended, to
add this earth to graves, in instances in which a rapid decay is con-
sidered desirable—as on the occasion of the crowding of grave pits,
with dead bodies, during the prevalency of pestilential diseases.[1]

[1] The following instance, extracted from the ninth volume of the Philoso-
phical Transactions, abridged, may be given as an example of the vague,
and, as I believe, erroneous manner of considering the operation of lime. In
1746, when means for preventing the infection of an epidemic disease, which
then prevailed among the cattle, were under consideration, burying them
was thought the most effectual method, and the introduction of lime was re-
commended "for the more speedy destruction of the distempered carcases."
But doubts arising "whether the lime might not exalt the putrid particles,
and help to spread the infection, it was the opinion of several of the learned,
that it was most safe on that account to bury them without it."

Dr. Parsons, the author of the paper, adds, "that the question will proba-
bly be decided by a fact that had come to the knowledge of one of the jus-
tices, John Milner, Esq., appointed to inspect into the affair, and will serve
to prevent the practice of burying them with lime for the future, as it makes
it more than probable that malignant particles (by the operation of the lime)
may be sent up, and spread through the air."

The fact referred to was the following: "Mr. Stallwood, a farmer, at
Hackney, informs the justices to whom the care of the distempered cattle
was committed; that he had buried thirteen cows, very deep, with the quan-
tity of lime appointed by the justices; and, observing his dogs to scratch, and
tear up the ground with their feet, to get at the cow's flesh, (the lime fer-
menting and causing a foam as he called it, or strong scent of meat to arise,
which made the dogs so eager to come at it), he beat them off several times;
but the dogs always returning, as soon as he was gone, for some time he
hired a boy to keep them off. But that he had buried several other cows in
another place, with their hides cut and slashed, without any lime, and the

From the results of many experiments which I have made with lime, on animal substances, I have been compelled to come to the conclusion, that this opinion is not well founded in fact—indeed, that it is altogether erroneous.

The experiments were commenced in Malta, in the summer of 1829, and they were carried on during the following year. The method observed, was to immerse the animal matter for trial, in cream of lime, or rather a paste of lime, contained in a wide-mouthed bottle, well corked, and covered with cerate cloth, to exclude the ingress of atmospheric air, and so preserve the lime in its caustic state.

One of the first experiments tried was commenced on the 27th August. A portion of muscle, rectum, verge of anus, urinary bladder, prostate gland, vesiculæ seminales, a portion of jejunum, the gall-bladder, and the gall-ducts, were immersed as mentioned above. They were taken from a subject in a state of incipient putrefaction, and they exhaled a fetid smell. On immersion in the lime and water, as might be expected, they gave off a strong ammoniacal odour. They were first examined on the 24th of September. They were then all in excellent preservation ; swollen, but not corroded, nor their delicate tissues injured. They were next examined seven months after, viz. on the 5th of May of the year following. The report was equally favourable; it is stated that they were much in the same state as before—the texture of each part distinct—and the part, as a whole, easily distinguishable. They were left undisturbed nearly two years—until the 6th April, 1832, when, on examination, they were found to have undergone material change. The cuticle had become soft and transparent, as had also the dura mater, admitting of being torn with the greatest ease. The muscle appeared to be converted into adipocire, which was quite white— had no unpleasant smell—was friable when dried, and burned with a bright flame, without any unpleasant smell. The other parts were not distinguishable.

The second experiment recorded, was commenced in the beginning of October. Portions of aorta, dura mater, intestine, skin, cellular tissue, muscle and tendon, were similarly treated. The results were examined on the 5th of May following. Then, on opening the bottle, an ammoniacal but no putrid smell was perceptible. The parts were found well preserved, excepting the fatty matter contained in the cellular tissue, which had become of an opaque white and friable, from combination with the alkaline earth and conversion into soap. The tendon, it is mentioned, was somewhat distended and

dogs never attempted to scratch or tear up the ground there." Two bushels of lime were allowed to each cow. With lime, the bodies were buried ten feet deep; without lime, eight feet deep.

Relative to the explanation of the fact—was not the difference observed in the two instances owing to this—that in the one the dogs were attracted by the smell of the meat preserved by the lime—and not in the other, where it was not so preserved, and where it was undergoing putrefaction?

rendered more transparent, but not gelatinised; and so also, in a less degree, were the dura mater and cutis; and the last was deprived of its cuticle and hair.

Some other experiments were made, but, as the results were very similar, it would be tedious to describe them. I may state, generally, that with the exception of cuticle, nail, and perhaps hair, lime exerted on the different textures, on which it was tried, no destructive power, but, a contrary influence—and more particularly a well marked antiseptic one. It has been stated how certain parts, in the first experiment, lost the putrid odour which they had acquired, when immersed in lime and water. Moreover, it appears from notes of experiments, that after animal substances have been fully subjected to the action of lime, they ceased to be putrescent; they resisted putrefaction, whether placed in air, or plunged and kept in common water. I shall mention one instance. On the 13th of May, 1830, a portion of ileum, with mesentery attached, and a portion of muscular part of heart, with chordæ tendineæ, were placed in a large jar of transparent lime water, and covered with cerate cloth. Examined nine months after, on the 16th of February, they were found in good preservation, and without any putrid or unpleasant odour. The only change perceptible was, that the portion of heart and intestine had acquired a light greenish hue, and the tendon an opalescent hue; and all were a little softened. A crust of carbonate of lime had formed on the water, which still retained some caustic lime. They were then transferred to a jar of common water—where, after four days, they continued unaltered. I may add, that a portion of cutis, similarly treated, placed in confined air in a bottle, after a whole month, emitted no unpleasant odour, and appeared to be unchanged.

I have observed, that cuticle, nail, and perhaps hair, are to be excluded from the list of animal substances, not materially altered by the action of lime. On the cuticle its action is powerful, and, I apprehend, in consequence of the chemical combination between them being formed. It is well known how lime has the property of rendering the cuticle easily separable from the cutis vera, and how, in the art of tanning, it is applied to this purpose. The human cuticle, that, for instance, of the sole of the foot, I find, becomes soft and gelatinous from immersion in lime and water. After drying, a portion thus tried (well washed previous to drying) was white, semi-transparent and brittle; incinerated, it yielded seventeen per cent. of ash, which consisted principally of lime and carbonate of lime.

The effect of lime on nail is similar to that which it exercises on cuticle, but not so strongly marked. A portion of nail of great toe, macerated in lime and water, from the 7th of June to the 18th of August, was rendered soft and friable—a little swollen and disposed to separate or break up in layers. Dried, it exhibited the same character as cuticle, and when incinerated, burned in a similar manner, and left a considerable ash, consisting of a small proportion

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of phosphate of lime, which pre-existed in the nail, and a large proportion of lime—with which, during the change from maceration, it may be inferred, it combined.

On hair, the effect of lime appears to be more destructive; but, in what manner it acts, I have not attempted to ascertain. A portion of human hair of the head, which had been kept in lime and water about three months, was partially decomposed. At the bottom of the vessel there was a little black sediment. The hair, which was black, had acquired a just perceptible reddish shade, and had become much finer, as if wasted, and more feeble, so as to be easily broken.

Relative to the results of the experiments generally, they appear to me to bear me out in the remark with which I prefaced them, viz. that lime does not exercise a destructive corroding power on animal substances generally, or one promoting their decomposition; but, on the contrary, a preservative and decidedly antiseptic power, arresting putrefaction, even when commenced, and retarding decomposition. What new arrangements of the elements of animal matter may take place under the influence of lime, is a subject for further inquiry. Probably the effects of lime on cuticle, nail, and hair, on which in the arts its operation has been best known, led to the ideas of its agency on animal substances generally, which I have been under the necessity of combating.

It was my intention to have instituted a set of experiments on the effects of the other earths, and also of the alkalies on animal textures; but this I have only very partially accomplished.

From the first few trials which I have made with barytes and magnesia, the action of the former on animal substances generally, and also on the cuticle and nail appeared to be similar to that of lime, but more energetic; whilst that of the latter appeared very much less, not arresting the putrefaction of the soft parts, and having, if any, an obscure and slight effect on cuticle and nail.

The effects of the alkalies, as might be expected, were sufficiently well marked.

Ammonia, in the form of aqua ammoniæ, acted very like lime. Portions of skin, of sole of foot, nail, hair, intestine, dura mater, after having been a fortnight in this liquor, had very much the same appearance as if they had been immersed in lime water. The cuticle was detached; it was quite pulpy; it offered no resistance to pressure, and in consistence was not unlike starch gelatinised by cooling. The other parts, not including the nail and hair, were rather soft and swollen, but not materially changed; the hair and nail showed no alteration, with this exception in regard to the latter, that its inferior stratum—its under surface, was softened, like cuticle. After another fortnight, the cuticle was found to be quite dissolved; the other parts, including nail and hair, remaining as last noticed: nor after another month were they found further altered, excepting that they were softer, especially the skin, which had become very flexible, and no longer retained the hair.

Weak solution of potash, in the form of aqua potassæ, tried on the substances last noticed, had very little effect on any of them; it prevented indeed their putrefaction, and softened them, but no more; after a fortnight, they appeared unaltered, and the cuticle was not detached.

A strong solution of potash, formed in the proportion of a dram of the hydrated alkali to an ounce of water, acted powerfully on them; the cuticle was first rendered gelatinous, and afterwards reduced to a blackish powder, as if entirely decomposed, and after about a month's action on the other parts, they too had disappeared, a little viscid mucus-like matter having taken their place, and a black matter subsided. These few experiments on ammonia and potash were made in Malta, in the summer season, at a temperature between 80° and 90°.

XII.

ON THE EFFECTS OF TANNIN ON THE DIFFERENT TEX-TURES OF THE HUMAN BODY.[1]

The question, are there any other textures besides the cutis, which have the power of combining with tannin,—led me, whilst stationed in Corfu, and in Malta, in 1837-38, to institute some experiments on the subject, believing that there would be no difficulty in arriving at satisfactory results, and hoping, that even unexpected results, as so often happens in experimental research, might be obtained, which might prove both curious and useful.

The first set of experiments I undertook, was commenced on the 1st of November, of the year already mentioned, at Corfu. The parts employed were from the body of a young man, who had died of remittent fever; they were kept in a tanning infusion until the 23d of June. The second set of experiments entered on, was commenced in Malta, on the 15th of April following. Most of the parts employed were from the body of a man, aged thirty-five years, who had died from the effects of a severe injury, occasioned by a fall; they were kept immersed until the 4th of July.

The parts subjected to the experiments, the instant they were separated from the body, were deprived of superfluous moisture, by pressure between folds of blotting paper, and then carefully weighed, and immediately after put into a strong infusion of catechu, with some catechu in powder. In every instance, putrefaction to a certain extent took place; after a while it diminished and ceased. The

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[1] First published in the fourth volume of the Transactions of the Edinburgh Med. Chir. Society.

infusion was changed once or oftener in the course of the experiment, according to circumstances; or, if the part was very small, not at all. The experiments were continued till a full effect appeared to be produced; to ascertain which the parts were occasionally taken out and examined, and partially dried by blotting paper, and weighed. The following table shows the quantity of solid matter per cent. which each afforded, when dried on a vapour-bath, as in the experiments on desiccation. The increase of weight it was supposed, when it could be clearly established, would serve to indicate the quantity of combined tannin.

No.	Part tried.	Residue per cent.
1	Skin of thumb, afforded	55.6
2	Cuticle of sole of foot, most of the cutis removed	53.4
3	Pleura costalis	83.3
4	Lower part of ileum	38.5
5	Upper part of jejunum	34.4
6	Aorta	52.3
7	Muscular part of diaphragm, neither its peritoneal or pleural lining removed	55.3
1	Rib, afforded	58.8
2	Cartilage of false rib	57.0
3	Intervertebral substance	41.7
4	Dura mater	53.0
5	Pia mater	53.4
6	Cellular structure	69.3
7	Perichondrium of cartilage of false rib	72.2
8	Cornea	55.5
9	Sclerotica	70.0
10	Corpus cavernosum penis	45.7
11	Medulla oblongata	43.4
12	Pectoralis major muscle	36.4
13	Aorta	47.1
14	Vena cava	40.2
15	Thoracic duct	53.5
16	Liver	27.6
17	Spleen	28.3
18	Lung	23.1
19	Kidney	17.4
20	Pancreas	16.4
21	Fibrinous concretion from ventricle of heart	45.6

I have stated, that putrefaction took place in every instance, in a greater or less degree, and that after a time, on changing the infusion, it ceased. Some animal matter, too, in each instance, was separated from the part, and deposited. At the same time the infusion of catechu had its strength impaired; some of the catechu appeared to be precipitated in the sediment, a part of it in most instances seemed to combine with the texture, and another part of it rose to the surface, in the form of a thick pellicle having undergone some change.

The animal parts which seemed to suffer most from putrefaction,

were the liver, spleen, pancreas, and kidney. The cuticle was the only part that did not appear to combine in the smallest proportion with tannin, or to be preserved by it. After long immersion in the infusion, it was as it were rotten, the particles of it hardly held together, and were most easily detached, and, when dried, their cohesion was almost entirely destroyed. This, however, strictly applies only to the outer layer, not to the inner, and there appeared to be a gradation from it. The nearer it approached the cutis the more coloured it had become and compact, and the more it seemed to partake of the nature of this membrane, when converted into leather. All the other parts when more or less indurated, and rendered more compact and firm, and all of them were more or less coloured. When dried, they were all comparatively rigid, with the exception of the pleura and pia mater. When immersed in water, they imparted to the water a brown colour. The colouring matter thus withdrawn when exposed to the air was gradually precipitated, and became insoluble like vegetable extractive, and when burnt it emitted a mixed fume of vegetable and animal matter,—the former predominating; and, by its abstraction, though the colour of the different textures was very little altered, yet they were all rendered more brittle, even the pleura, when dry, lost almost entirely its flexibility. I tried the effect of water in this manner only on the parts employed in the first set of experiments. The loss each part sustained, per cent. when dried on the vapour-bath, was as follows:—

No.	Part tried	Per cent.
1	Cuticle of sole of foot	21.8
2	Skin of thumb	7.3
3	Pleura costalis	25.0
4	Lower part of ileon	9.7
5	Upper part of jejunum	9.0
6	Aorta	6.0
7	Muscular part of diphragm	3.4

The great loss sustained by the cuticle was probably chiefly in consequence of the tannin and colouring matter which it had merely imbibed, when distended by the infusion like a sponge, being washed out, and from some of its particles having been detached, in consequence of its decayed state already alluded to. None of the parts showed the least tendency to putrefy during this immersion in water, though the temperature of the air at the time was high, often above 90°, and they were exposed to the air.

Considering all the circumstances of these experiments, the loss of a portion of animal matter from putrefaction,[1] the apparently

[1] I believe the degree of putrefaction and effect produced by it, is in some measure proportional to elevation of temperature, and that this is a principal cause that the leather is so well fitted for the climate in which it is made. Made in a hot climate, it is soft and very porous, allowing heat and perspi-

great increase in weight in several instances, and the change of
properties produced in all, tending to the preservation of the part,
with the exception of the cuticle, I think it may be fairly inferred,
that the cutis is not solitary in its power of combining with tannin
and forming leather; on the contrary, that the fibrin of the blood,
in the form of a fibrinous concretion, or the buffy coat; and that the
fibrous and serous textures, and the vessels, including each kind,
possess this power in a still more eminent degree,—and that the mu-
cous membranes possess it in an equal degree; and may not the same
conclusion be extended to muscular fibre ?   Relative to the liver,
spleen, pancreas, and kidney, their power of combining with tannin
is problematical.   That a portion of each of these viscera does com-
bine with it and the colouring matter of catechu, is pretty certain,
and most likely it is the connecting cellular structure which enters
so largely into the composition of their substance. I am disposed to
think that this remark does not apply to medullary matter; it seems
in a certain manner to be preserved by tannin, without entering into
distinct combination with it.

After having obtained the preceding results, the idea naturally
occurred that other textures of animals besides the cutis, might be
subjected advantageously to the process of tanning for various uses.
It seemed not improbable that some parts might be useful in the
form of tubes, as the intestines thus converted into leather. Farther,
it seemed highly probable that the art of tanning might be applied
to the making of certain anatomical preparations, in a dry state, for
the purpose of public demonstration, and that it might also be turned
to account in the art of embalming.   The few experiments which I
have hitherto made in relation to these applications, are as favoura-
ble as could be expected, and make me sanguine, that whoever
undertakes to push them farther, and give them a fair trial, will be
eminently successful.   Some preparations which I have made of
the larger blood-vessels and of the intestines, display their parts per-
fectly in a dry state, and when immersed in water and distended by
the absorption of this fluid, they even display very well the differ-
ent coats of which they are formed.   I suspect that, anciently in
Egypt, tannin was employed in embalming.   Some years ago, I
recollect having examined a heart, and the fluid in which it was
found preserved, in an alabaster vase, lined with a substance like
petroleum.   It had been brought, I believe, from Thebes, in Upper
Egypt, and, if I am rightly informed, it is deposited, by Dr. Bree, to
whom it belonged, in the Museum of the College of Physicians, of
London.   The heart had the appearance of being tanned, and the
fluid in which it was preserved was chiefly water, holding in solution
some animal matter and a vegetable principle, analogous to tannin,
both in very small quantity.   It is probably the most ancient moist
preparation known, and it is well adapted to show the preservative

ration to escape; made in a cold and temperate climate it is strong and dense,
excluding moisture and retaining heat.

powers of a fluid ; and that if air be excluded, there is scarcely any limit to the durability of bodies so protected.

XIII.

ON THE EFFECTS OF BOILING WATER, AND OF BOILING, ON THE TEXTURES OF THE HUMAN BODY AFTER DEATH.

The majority of the experiments which I have to detail on this subject, were made in Malta, in 1828–29. They were instituted chiefly for the purpose of testing in part the correctness of Bichat's theory of membranes,—in the developement of which, in comparing different textures, he lays much stress on the effects in question.

I shall first notice the effects witnessed from the application of boiling water to portions of different textures, of small bulk, so small as to have their temperature almost immediately elevated to that of the scalding medium. The trials were made in a very plain and easy manner. In each instance, the animal matter was placed in a Wedgewood-ware evaporating dish, and water boiling from the fire, in a common tea-kettle, was poured on it, in a continued stream, for a few seconds, until its own temperature, it was supposed, was imparted.

1. Cerebrum, medulla oblongata, deprived of their membranes, did not appear to contract or undergo any change of form, neither the cortical nor medullary part : they were rendered, however, weaker, as if the cohesion of their particles was diminished,—giving way when an attempt was made to raise a portion with the forceps; and yet they were not sensibly softened.

2. Muscle ; a portion of gastrocnemius contracted pretty much, and apparently in all its dimensions. Portions of other muscles were tried, and with like effect. At the same time, that they appeared to be condensed, they became comparatively pale; and also rather hard, seeming to retain their strength of fibre.

3. Liver, spleen, kidney, lung ; each of these experienced contraction, and all of them became friable. The portion of lung, when covered with hot water, quite effervesced, so much air was disengaged, partly, no doubt, from the expansion of the contained air from heat, but, chiefly probably from contraction of the air-cells.

4. Cartilage ; a superficial slice of the investing cartilage of patella, contracted pretty much and became bent, forming nearly half a circle : the next slice contracted and was bent much less, giving rise to the idea that the greater effect on the former was owing to its possessing a delicate membrane, liable to greater contraction from heat, than the substance of cartilage alone.[1]

[1] There is a preparation, in the Museum of the Army Medical Department,

5. The tendo achillis became shrivelled and appeared to contract in all its dimensions.

6. The dura mater became corrugated and appeared to be much contracted; from shining silky white, its natural colour, it became dull white, with a yellowish tint.

7. The pericardium became shrivelled and appeared to be contracted; its inner surface exhibited corrugation, and seemed distinctly fibrous; there was no appearance indicative of its having a serous membrane.

8. The skin, contracted very much; the surface of the cutis vera became rough, as if glandular; the cuticle was rendered weak, almost pulverulent, scarcely offering any resistance.

9. The transparent cornea became corrugated and apparently opaque.

10. Stomach and intestines; their lining membrane was very much contracted and corrugated, and so altered in appearance, as not to be distinguishable; it was rendered also very weak, so as to be easily rubbed off. The same changes occurred in the peritoneal and muscular coat in a less degree. The glandular structure, especially of the duodenum, appeared more distinct.

11. Aorta; its outer membrane contracted and was very much corrugated; its inner, [tried when detached,] very slightly, and its strength appeared to be very little diminished; the middle layer curled a very little, and uncurled when put into cold water,—and its strength was not apparently diminished.

12. A portion of vena cava became condensed and corrugated, curling, spirally, outward, from the greater contraction evidently of the outer fibro-cellular coat; the wrinkles appeared in the inner coat; there was no distinct loss of strength. A portion of the vena portæ, was very much corrugated; one portion of its inner coat was suddenly rendered gelatinous; another and contiguous portion, harsh and friable.

13. The thoracic duct contracted, and was very much shrivelled.

In a few instances I endeavoured to determine, by measurement, the degree of contraction. A portion of dura mater, after the action of boiling water, was found contracted about one half its length and width; but increased in thickness; a portion of tendo achillis gave nearly the same results; a small fasciculus of fibres, from gastrocnemius externus muscle, contracted less in length, only about one third. To try to determine whether the contraction, in certain directions, was connected with a general condensation of substance, I ascertained the specific gravity of some of the textures before and after the scalding agency, subjecting them, on each occasion, as nearly as possible, to the same degree of pressure between folds of

at Fort Pitt, made by Mr. Gulliver, clearly demonstrative of the existence of such a membrane, extended from the synovial fringes over the cartilage in question. In an instance of partial absorption of the cartilage of the patella, producing a small cavity, I found it covered with a delicate membrane, the normal one, I believe, unaltered.

blotting paper before weighing them. The results obtained are shown in the following table.

Texture tried.	Specific Gravity before scalding.	Specific Gravity after scalding
Spleen	1041	1060
Dura mater	1047[1]	1058
Pericardium	1059	1074
Lower portion of thoracic aorta	1065	1075
Ditto, just below the arch deprived of its outer coat.	1070	1072

These results would seem to show that condensation attends the contraction,—that the bulk of the part acted on is actually diminished. This is also indicated, and I apprehend very clearly, by the effect on certain parts,—especially on the penis,—which, perhaps from its structure, is better adapted than any other to display it. Under the influence of boiling water, this organ becomes hard, and tense, and rounded; and from the blood *pressed out*, evidently of diminished volume.

Before passing to another part of the subject, I may mention a small number of trials which I made to endeavour to determine at what temperature the corrugating power of hot water commences. Dura mater was selected for the experiment. Water of the temperature of 140°, poured on it, had no apparent effect; at 150° it was doubtful; at 155°, there was a perceptible corrugating effect; at 170° it was considerable; and at 212° the effect was at its maximum.

I shall now describe the effects witnessed on different textures from the operation of long continued boiling. As a preliminary, the effects of this agency on the principal constituent parts of the blood, or those into which it is resolved, when abstracted, may be mentioned.

1. Some serum of blood was coagulated by immersion in boiling water. The coagulum was inclosed in a linen bag and kept in boiling water over a slow fire, for twenty-four hours. The matter of the coagulum was not in the least softened; showed no tendency to become gelatinous; it was firmer and more brittle than when first coagulated, breaking with a conchoidal fracture. Wiped with blotting paper to remove adhering moisture, it was found to be of specific gravity 1031. This low specific gravity, perhaps, might have been expected, as when the serum was first caagulated by heat, there did not appear to be any condensation; the vessel which was filled with the serum, was equally filled with the coagulum; nor from long boiling was there any very distinct appearance of contraioctn.

2. A portion of fibrin of blood, the colouring matter and the serum separated by washing, was plunged into boiling water; it

[1] This low specific gravity of dura mater, was probably the effect of disease; in the body from which it was taken, fluid was found effused between the membranes.

immediately contracted, and apparently very much. The water was kept boiling for twenty-four hours. The fibrin was not rendered gelatinous; nor did it appear diaphanous in the slightest degree. It was not entirely deprived of its elasticity; it was perhaps a little softer, but not distinctly so. Pressed between folds of blotting paper, it was found to be of specific gravity, 1067.[1]

3. A mixture of red particles and serum, after the separation of the fibrin, by being pressed through linen cloth, poured into a vial, was immersed in boiling water; the cruor immediately coagulated pretty firmly. The water was kept boiling for 24 hours. The coagulated cruor was rendered softer and more friable than the coagulum of serum; its colour was a dull brick red, which it had after the first impression of the heat. Pressed between folds of blotting paper, it was found of specific gravity 1090.

In the following trials the operation of boiling was carried on uninterruptedly for ten hours. The parts subjected to it were immersed in the water already in ebullition. They were all from a body 21 years of age at the time of death. It may suffice to state briefly the principal appearances observed in each texture, or in each compound organ, at the end of the operation.

1. Cerebrum.—Both medullary and cineritious matter, seemed rather contracted, was rather harder and more friable, and felt greasy.

[1] In another trial, made on a fibrinous mass from the right ventricle of the heart, boiled for about twelve hours, its specific gravity was found to be 1049.

Considering the effect, above described, of a boiling temperature many hours continued on the fibrinous part of the blood, it is very curious that coction or digestion of fibrin at a comparatively low heat, viz. about 99°, should afford a very different result: thus, if the blood-heat coction be continued for 48 hours, without stop, the fibrin is reduced to a semi-fluid state, coarsely resembling pus, and very closely resembling the puriloid matter into which fibrinous concretions formed in the heart, or blood-vessels, during life, are in part converted under peculiar circumstances. To Mr. Gulliver I apprehend belongs the merit of having first observed this peculiar effect of coction at a blood-heat on fibrin, and the analogy between its product and the result of the softening process on fibrin in the living body.* The analogy, however, it is right to observe, is not altogether perfect. In Mr. Gulliver's experiments on the coction of fibrin, I believe, without any exception, marks of putrefaction were developed in the fibrinous mass, which may conduce to the disintegration of the fibrin, and its reduction to a pultaceous state; farther on, it will be seen how rapidly at a temperature between 80° and 90°, fibrin partially liquefies in undergoing putrefaction; but in the fibrinous concretions, when undergoing softening, I have never observed any well-marked signs of incipient putrefaction; the softened matter has never, that I have witnessed, emitted an offensive smell, and when it has been mixed with lime or caustic potash, it has yielded a very slight odour only of the volatile alkali. The softening of the fibrin, in the living vessels, *centrically*, [as is invariably the case, as far as my experience extends,] whilst the surface of the mass is hardened so as to assume the appearance of a sac or inclosing membrane, does not seem to admit of explanation on any known principle: it is an important problem in relation to its pathological relations, and deserving of minute inquiry.

* London Medical Gazette, vol. i. New Series, p. 874.

2. Dura mater, was thickened, rendered more transparent and elastic; it was easily stretched and broken. Its outer surface was shrivelled. It admitted of being easily divided into two laminæ; the exterior of which was thinnest. The direction of the fibres was horizontal. No appearance of arachnoid membrane, or of a membrane reflected over its inner surface was perceptible.

3. The external investing membrane of the heart was rendered soft and imperfectly gelatinous.

4. The lining membrane of the cavities of the heart was rendered slightly gelatinous.

5. The cordæ tendineæ were rendered soft, not in the slightest degree viscid, but very friable; they were pellucid, and like cold jelly.

6. The valves appeared contracted and corrugated; they were very friable, and slightly gelatinous; their base was more so than their free margin.

7. The muscular substance of the heart was rendered very friable and soft. It showed a disposition to come away in spiral masses, when pulled asunder.

8. The outer coat of the pulmonary artery and of the aorta was rendered softer, and somewhat gelatinous, and slightly viscid, so as to be slightly ropy when drawn out. It could be traced down to the valves; showing, as it appears to me, and as is confirmed by the effect of maceration, that the semilunar valves of both great arteries are connected in part with this outer coat.

8. The middle coat of both vessels was rendered more friable, but not transparent, and not in the least gelatinous:[1] it was much less weakened and altered than muscle.

9. The inner coat too was rendered more friable, more difficult to separate as a continuous membrane, indeed it could be separated only in fine shreds, and it did not appear at all gelatinous.

10. The pleura was rendered soft, rather gelatinous and a little viscid.

11. The substance of the lung had become firm, compact, very friable, and of a greasy feel.

12. The liver, kidney, and pancreas, were rendered very similar; and their investing membrane had become very like the pleura.

13. The inner coat of the stomach and intestines was very much corrugated; it had not become in the least gelatinous, but very friable and easily separable from the cellular coat by rubbing.

14. The cellular coat, between the inner and muscular, was rendered very soft, somewhat gelatinous and viscid: it felt granular, as if it contained mucous glands.

[1] I have found the effect of long-continued boiling on the substance of the ligamentum nuchæ of the ox similar, and also similar on the thick, very elastic middle coat of the aorta, as in the arch, where this structure is most strongly marked in the ox.

15. The muscular coat had become very friable; not at all gelatinous; the muscular fibres were very distinct.

16. The peritoneal coat generally was rendered soft; in some places it was rather gelatinous and slightly viscid, in others friable.

17. The inner coat of the urinary bladder and of the upper part of the urethra, had become slightly gelatinous and diaphanous.

18. The muscular coat of the former was delicately displayed; its fibres appeared very distinct; they were friable.

19. The cuticle of the tongue was rendered soft and very friable, and was very easily detached. It was thick above and spongy; extremely thin below, and over the frænum indistinct. I attempted to trace it in continuation. Over the cryptæ of the amygdalæ I could not detect it; nor in a satisfactory manner in the pharynx, in the immediate vicinity of the amygdalæ. The cryptæ and contiguous lining membrane had become gelatinous. In the œsophagus, the cuticle was very manifest, easily separable, indeed scarcely adherent, and very friable.

20. The cutis of the under surface of the tongue, extremely thin, was rendered slightly gelatinous and semi-transparent. The thick cutis of upper surface had become rather friable, especially the papillæ, as if composed chiefly of vessels. When it was removed, gelatinous projections were seen rising from the muscular coat corresponding to the papillæ.

21. The mucous coat of œsophagus had become rather friable; some gelatinous matter appeared to be diffused through it. The sub-mucous, cellular, and the mucous coats were affected much in the same manner as those of the stomach and intestines.

22. No cuticle or epithelium was discoverable in the larynx, or in any part of the aspera arteria. It was very carefully sought after, on the supposition, that it might exist of a very delicate nature.

The lining membrane of the larynx was found to be semi-transparent, elastic and gelatinous; the cordæ vocales were similar, as also the connecting membrane of the cartilages of the trachea.

23. The cartilages of the larynx had become much swollen and broken; portions of them were projecting out. They were very soft, nearly transparent, and quite gelatinous. The change in the epiglottis was less strongly marked than in the firmer cartilages; it was less uniformly swollen, soft, and transparent. The perichondrium generally was rendered gelatinous.

24. The cartilage of patella was found soft and semi-transparent, like stiff jelly.

25. The lining membrane of the capsule had become soft, rather gelatinous, and slightly adhesive.

26. The cellular membrane, surrounding the bursa, between the patella and the skin, was found soft, gelatinous, and ropy, admitting of being drawn out into threads possessing some elasticity.

27. The cutis over the patella was much corrugated; it had become soft, friable, and a little gelatinous; one surface was more

gelatinous, the other more friable. The outer surface was rough, from little protuberances of a gelatinous kind.

28. The skin of the scrotum appeared somewhat similar, but without the insulated little masses of jelly on its surface.

29. Its cellular membrane had become soft, gelatinous and ropy. The tunica vaginalis was very similar; it was equally gelatinous and soft.

30. The albuginea was rendered soft and friable, not viscid. In appearance, it approached somewhat to the cartilage of patella, but was less transparent.

31. The testicle was found firm and very friable.

32. The conjunctiva of eye had become rather gelatinous, soft, and adhesive.

33. The cornea was rendered translucent, and friable, like the albuginea.

34. The sclerotic coat was similar: the cornea and this coat were not separable.[1]

35. The lens had become opaque white, firm and very friable, —not in the slightest degree gelatinous: it broke up into concentric laminæ.

36.[2] The articular cartilages investing the ends of the bones of the foot were rendered semi-transparent; soft, like stiff jelly.

37. The tendons of the foot had become similar; the tendo achillis was also gelatinous, but less completely so; there were whitish portions observable in it, as if from minute vessels.

38. The pancreatic duct was found changed in a manner resembling tendon; it had become uniformly transparent and gelatinous.

39. The outer coat of the ductus communis choledochus, was rendered very like the corresponding coat of the aorta; its inner coat was less altered; it had become diaphanous, slightly elastic, and imperfectly gelatinous.

40. The cutis of body of penis had become soft, gelatinous, and elastic.

[1] An eye, an inch in diameter, measured from the entrance of the optic nerve to the cornea, and transversely, immersed in boiling water about ten minutes, was found contracted to a diameter of $\frac{46}{50}$ths of an inch; and the cornea from $\frac{24}{50}$ths to $\frac{22}{50}$ths; the latter was not corrugated or diminished in transparency. The boiling was then continued about ten hours; the globe was still more contracted, but the sclerotic having burst, it was not easy to measure it; the cornea was reduced to $\frac{17}{50}$ths of an inch. It and the sclerotic had become soft, not gelatinous; the one retained a considerable degree of transparency, and the other had acquired it.

[2] This and the following trials were made on parts of a limb, amputated on account of extensive disease of the knee joint, following a fall; the patient was 21 years of age. The amputated limb was boiled eight hours. It was curious to see the general disruptive effect. All the bones of the feet were dislocated; the tendons broken; the cuticle almost entirely detached. The toes and metatarsal bones were bent downwards.

41. The tendinous sheath of penis was affected very like dura mater; but was rendered softer; it admitted of separation in layers.

42. The corpus cavernosum and spongiosum had become very friable, slightly elastic, and only slightly diaphanous. No epithelium could be detected, continued from the cuticle of the gland into the urethra. The lining membrane of the urethra was rendered gelatinous.

43. The glans penis throughout had become soft, elastic and gelatinous. Its cutis was very gelatinous; minute papillæ, semitransparent points, appeared regularly distributed over it.

In many instances, the specific gravity of the parts was ascertained, after the operation of long continued boiling. The following table exhibits the results obtained. The number in the table, corresponds with the preceding numbers.

Table showing the specific gravity of different textures after long boiling.

No.	Description of Texture.	Sp. Grav.
2	Dura mater	1023
7	Columna carnea of left ventricle of heart	1052
8	A section of aorto (including all its coats) close to the coronary arteries	1061
12	Liver	1046
—	Lung	1061
—	Kidney	1071
—	Spleen	1070
19	Muscular substance of tongue	1062
23	Thyroid cartilage	1012
31	Substance of testis	1070
36	Cartilage of os calcis	1020
37	Tendo Achillis, close to its termination	1034
A	Cutis of heel[1]	1029
B	Cuticle from heel	1076
	Dura mater	1030
	Abdominal vena cava	1040
	Its outer coat detached	1034
	Thoracic aorta just below the arch	1068
	Duodenum close to common biliary duct	1050
39	Common biliary duct, with a portion of pancreatic duct attached	1038
40	Cutis of dorsum penis	1033
41	Tendinous sheath of dorsum penis	1044
43	Glans penis	1045

All these results, received even favourably, can be considered only as approximations. They would be more valuable for the purpose of comparison, had the specific gravity of each part been ascertained before being subjected to boiling. However, imperfect

[1] These parts, to which a letter is prefixed, were portions of the amputated limb: those without the prefix of a letter or number, were taken from a body aged about 21; the latter were boiled eight hours.

even as they are, I hope they may not be without use as data for reasoning on, in forming general views of the analogies of textures.

The results generally are not so accordant with those of Bichat, as described by him in his Anatomie Generale, as might be expected : nor are they, in my opinion, favourable to his views of distinct differences in the qualities of certain membranes, which characterise his theory. He states that cellular membrane, and fibrous membrane are very differently affected by coction. In my trials it did not so appear. Judging merely from the effect of coction, the conclusion, which seems most legitimate is, that which harmonises with the views of Haller,—that the majority of the white membranous and fibrous parts, are of analogous composition, differing rather in degree than kind ; and not essentially different from cellular tissue ; and each differing in like manner in degree in different situations, so as not to allow of any individual membrane or continuity of surface throughout its course being considered identical. Farther on I shall have occasion to recur to this subject.

When engaged in this inquiry, I had intended to investigate more in detail the effects of coction, and I commenced making observations on the comparative tendency to putrefaction of the textures subjected to the boiling process. From the partial trials then made, it appeared that all those parts which were much softened and gelatinized by boiling, had become by the change much disposed to putrefy. It was remarkably the case in the instance of tendon, cartilage, and fibrous membrane. After being boiled, they underwent the putrid liquefaction even more readily than muscle : the effects in these instances, as in muscle, liver, lung, spleen, &c. in which, at least in the majority, there appeared to be an augmentation of specific gravity, rather than a diminution, was less clear : and I am doubtful, whether the coction really increased or diminished the putrefactive tendency.

A few experiments about the same time were made on the fitness of boiling, as an aid in the preservation of anatomical preparations. The result was favourable, showing that those parts which contain much blood, and which are themselves but little altered by a short application of the boiling temperature, may immediately be put into spirit of wine, and kept without its being necessary to change it. Thus, in February, 1829, a portion of blood coagulated by heat,—a portion of liver and spleen, merely indurated by immersion in boiling water, were placed in proof spirit. In May of the following year, they were found in excellent preservation, the spirit clear, without sediment, and only very slightly coloured.

XIV.

ON THE AGENCY OF ATMOSPHERIC AIR ON DEAD ANI-
MAL MATTER; AND ON THE PUTREFACTIVE PROCESS.

Amongst many questions of interest involved in the subjects
enumerated above, there are two which stand forward in a promi-
nent manner; viz. whether dead animal matter before putrefaction
commences has the power of absorbing air; and whether the putre-
factive change is attended with elevation of temperature.   Bearing
on these points, although not exclusively so, I shall relate three sets
of experiments which I have made: one on the agitation of fresh
animal matter in atmospheric air; another, on its exposure to this
mixed air, confined without agitation; and a third, on exposure to
the atmosphere, under circumstances favourable to putrefaction:
and, I shall conclude with some general remarks on the putrefac-
tive process.

My attention was first specially directed to the inquiry in Malta,
in 1829.   Of the results then obtained and published in the Edin-
burgh Medical and Surgical Journal, I shall now give only such
as, on revision, I believe to be correct, and liable to no serious ob-
jection.

1.—*On the agitation of dead animal matter in atmospheric air.*

The results of the latest experiments which I have made on the
agitation of blood and serum in air, have already been stated, show-
ing, that, independent of putrefaction, both venous and arterial
blood have the power in question; but that serum does not appear
to possess it, in an appreciable degree.   Respecting serum, I think
it right to express myself in this qualified manner in consideration
of the well established fact,—first, I believe, pointed out by Dr.
Priestley,[1] that venous crassamentum, when guarded from the
atmosphere merely by a thin covering of serum, acquires super-
ficially the arterial hue; indicating the passage, if not the absorp-
tion, of oxygen gas.

To try the effect of the solids,—two substances have been se-
lected as best fitted to give decisive results, viz. cerebral matter and
muscular fibre.   The experiments on the agitation of these sub-
stances have been made in a manner very similar to those described
on blood and serum.   The brain of a sheep was used—its masseter
muscles and tongue.   As soon as the animal was killed in the usual
way, its head was removed and brought from the slaughter-house.
The brain was extracted whilst still warm, and immediately im-
mersed in water, just in sufficient quantity to cover it: it was

[1] Experiments and Observations on different kinds of Air; by Joseph
Priestley, Birmingham, 1790, vol. iii. pp. 365, 370.

cooled rapidly by placing the vessel containing it in running water; and quickly broken up and reduced to the state of an emulsion. In this state, with as little water as possible, it was poured into the double-mouthed bottle of the capacity of 32 cubic inches, provided with stop-cocks and a bent tube, as already mentioned, and well shaken in atmospheric air for about two minutes. On turning the stop-cock, with the bent tube attached connecting it with water, the water did not rise in the tube,—on the contrary, a few bubbles of air escaped. The result obtained, using rather more than a quarter of a pound of the masseter muscles, cut into very small pieces under water, was similar; as it was also, employing the whole of the tongue divided in the same manner. The cerebral emulsion, and also the first mentioned muscle was allowed to remain in contact with the air in the bottle for nearly twenty-four hours, without further agitation; after which, on turning the stop-cock, there was a slight rise of the water in the tube; but, I apprehend, not more than might be fairly attributed to a change of temperature which had taken place in the air of the room in the interval; a fall, namely, from 55° to 51°. What was the kind of air disengaged, I did not attempt to ascertain. It might have been common air expelled, owing to expansion from perhaps a little heat produced by the friction of agitation. Be this as it may, the quantity expelled was so very small, that the determining of its nature would have been attended with extreme difficulty.

As regards the conclusion to be drawn from these experiments, the same I apprehend may be admitted as in the instance of serum; viz. that neither muscle nor the cerebral substance perfectly fresh, have the power of absorbing oxygen in an appreciable quantity; and judging analogically, it may be inferred, at least as highly probable, that the other textures less prone to change, are also destitute of this power.

2.—*On the exposure of dead animal matter to atmospheric air, at rest in confinement.*

As bearing on the subject of putrefaction, as well as in part on the last, in relation to the animal textures,—I shall give the particulars of many experiments made at Malta during the summer season, on the exposure of dead animal matter to measured quantities of atmospheric air, confined in jars over mercury, at a temperature between 80° and 90°, a heat at which animal matter, as is well known, is peculiarly liable to change and become putrid. I shall first notice the results obtained, using blood and its principal constituent parts, physiologically considered.

The blood subjected to experiment was venous blood. It was placed in a receiver over mercury in a measured quantity of common air, a few minutes after it had been drawn, having been quickly cooled by immersion in water. During the first twelve hours, the volume of air over it was not sensibly changed. Soon after there

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was a diminution of the bulk of the air, and this diminution gradu-
ally increased. When greatest, it was nearly equal to the volume
of the blood. This was accompanied by a change of form of the
crassamentum; it, as it were, dissolved away, and an apparently
homogeneous fluid was formed. And next there was an augmen-
tation of the bulk of the air. By examination at different times,
I found that the volume of air remained unchanged until putrefac-
tion commenced;[1] that carbonic acid was formed and absorbed by
the blood; that the diminution of the volume of air continued till
the blood was saturated with carbonic acid; and that its augmen-
tation was owing to further evolution of this gas, connected with
the formation of ammonia, and the progressive changes of the pu-
trefactive decomposition.[2]

The air over serum of blood confined over mercury in the same
manner, remained many days without undergoing any change of
volume; and so long, the serum retained its transparency. At
length a turbidness appeared in it,—putrefaction had commenced,
and the volume of air diminished, owing to the absorption of car-
bonic acid gas which had formed. When the serum was saturated
with carbonic acid gas,—the putrefactive process continuing, the
volume of air slowly increased.

Results very similar were obtained, with the colouring matter
alone; or (which is more correct) the mixture of serum and red
particles, obtained by pressing the crassamentum gently in linen.
The changes in this instance were much the same as in the pre-
ceding; they took place more slowly than when blood with all its
ingredients was employed, and less slowly than when the serum of
blood was used.

The changes exhibited by the fibrin of the blood were the most
rapid of all. In less than twelve hours it lost its firmness; a small
diminution of the volume of air had taken place; a little carbonic
acid gas was found in the residual air, and oxygen had disappeared
about equal in volume to the loss by absorption, together with the
acid gas generated. In twenty-four hours or little more, the fibrin
had become semi-fluid or pultaceous; the oxygen of the air had
disappeared entirely, or nearly so, and its place was supplied by
carbonic acid gas, excepting a diminution (which it may be inferred
was carbonic acid gas absorbed) very nearly equal to the volume

[1] This may appear contrary to what has been already stated relative to
the absorption of oxygen by blood previous to putrefaction: but is not in
reality; without agitation, the putrefaction of blood, I believe, commences
[at least at a high temperature,] before the absorption of air by it, is demon-
strable.

[2] The products of the putrefactive decomposition are, I believe, very much
modified by temperature; so that were the results obtained in summer and
winter compared, they would be found greatly to differ. At a low temper-
ature, as between $40°$ and $45°$, in many instances, I have found both the
blood of man, of the sheep, and of the ox, to decompose without evolving
any air: there was a continued absorption of oxygen, and the formation of
a large quantity of carbonate of ammonia.

of the semi-fluid formed. As the putrefactive process proceeded, the fibrin divided into two parts—one pultaceous and a little frothy, occupying the spot where the fibrin was first put,—the other liquid surrounding the pultaceous matter ; and the volume of air gradually increased, carbonic acid gas being evolved, attended with the formation of ammonia and the other changes belonging to advanced putrefaction.

I shall next mention the results obtained in similar trials on different textures taken from animals just killed, as the rabbit and ox, —or from the human subject a few hours after death.   Paying as much attention as possible to uniformity of temperature, and watching the experiments carefully, I have never been able to observe any immediate absorption of air. In several instances, as of muscle, liver, intestine, &c., there was no diminution but a progressive increase of volume from the evolution of air.   In the few instances in which a diminution of volume did take place, it was very inconsiderable ; and as this was accompanied by incipient putrefaction, and generally attended with the appearance of some carbonic acid gas, is there not good reason to infer, that it was entirely owing to the absorption of carbonic acid gas formed, especially as the moisture of the part or the fluid which it contained was sufficient to account for it, and the oxygen which disappeared was equal to the absorption added to the carbonic acid gas remaining ? In corroboration of this view, I may mention the difference of results obtained, when animal substances were exposed to putrefaction under increased and under diminished pressure by means of a column of mercury acting on the air, in which the substances were confined,—in one instance, in the closed end of a tube bent upwards ; in the other, in the closed end of a straight tube, both of course placed perpendicularly.   When the pressure had been increased by the weight of eight inches of mercury, and diminished by a counterpoise of sixteen inches, the results were well marked, especially when using very putrescible substances, as the fibrin of the blood and muscular fibre.   With the increased pressure, at a temperature of about 76°, fibrin being used, in twenty-four hours there was a small diminution of volume of the air, with just sensible marks of putrefaction having begun ; whilst with the diminished pressure, there was a small increase of volume and a manifest appearance of putrefaction having commenced.   Now, it follows, of course, that the carbonic acid gas first formed would be more powerfully absorbed, and retained with the increased than with the diminished pressure ; but if oxygen was absorbed chemically, there is no reason to suppose it would produce any difference of effect except in degree.

The experiments, of which I have just related the results in part, were, as already mentioned, all made at a comparatively high atmospheric temperature,—favourable to rapid putrefaction.   The degree of rapidity of change, I may remark, varied according to the nature of the texture.   Brain, muscle, spleen, liver, and the

substance of the glands generally, produced the most rapid change. In a few hours carbonic acid gas might be detected in the air exposed to them; and generally in less than twenty-four hours there was air generated, and an increase in the volume of the air. The soft white parts were next in degree of rapidity,—such as the cutis, periosteum, dura mater, and I may add, stomach, intestinal canal, vein, artery, and adipose structure.   Over these, in about twenty-four hours, carbonic acid gas could generally be detected in the air; and generally, in about forty-eight or seventy-two hours a little air was generated.   Tendon, intervertebral substance, carti-lage, cuticle and bone, were slowest in progress.   In two or three days carbonic acid appeared in the air in which they were con-tained; but many days elapsed before there was any increase of volume indicating the generation of gas, excepting the parts belonging to a young animal.   After the putrefactive process had commenced, it proceeded, although the oxygen of the atmospheric air was entirely consumed: carbonic acid gas continued to be dis-engaged, in which sometimes a little carburetted hydrogen might be detected, and sometimes a little sulphuretted hydrogen;—ammo-nia was constantly formed; but I have not been able to satisfy myself that any azote was either absorbed or evolved, at least in the early period.   The appearance of carburetted hydrogen was most striking in the instance of muscle; it occurred irregularly; sometimes early in the process, more frequently at a later period; one day, it might be detected in the air generated, not in the next. A softening of the parts, like a deliquescence, always marked the progress of the change, and most of them became pultaceous. After about a fortnight or three weeks, brain, muscular fibre, the parenchymatous substance of the glandular structures and the spleen, were reduced to a very soft state, and on washing them, the soft matter, the result of putrefaction, was removed, and the vascu-lar structure of the part well displayed; and in the instance of muscle, the tendinous attachments also, and the aponeuroses: and these in their turn yielded and became decomposed, the process being allowed to continue, as I shall have a better opportunity of describing in the next section, when noticing the same changes taking place in the open air.

3.—*On the exposure of dead animal matter to the atmosphere, under circumstances favourable to putrefaction,—with a view to the question, Is the putrefaction of animal matter attended with an elevation of temperature?*

I shall give in some detail the experiments which I have made to endeavour to determine this question.   It may be premised, that they were all made at the same time and place as the preceding; that the subjects of them, as soon as possible, after they were taken from the body, were put into glass vessels, lightly covered with, and muffled in dry flax, for the purpose of diminishing the influ-

ence of the atmospheric air, as a cooling medium ; and that a vessel holding water, was placed contiguously, for the sake of comparison in relation to temperature. I shall commence with the blood and its different ingredients or parts into which it may be separated out of the body ; and from them proceed to the solid textures.

Blood.—On the 19th August, about a pound and a half of venous blood, taken from a young man, labouring under a slight febrile ailment, moderately firmly coagulated, having been abstracted about three hours, was broken up, so that the colouring matter, the fibrin and serum were well mixed, and immediately put into a glass vessel in the manner premised for observation. On the 20th at noon, its temperature did not differ from that of the water contiguous. At 6 p. m. of the same day, when the temperature of the water was 78°, that of the blood was 80°, and its odour was peculiar and rather offensive. On the 21st, when the water was 79°, the blood was the same as the day preceding. On the 22d, when the water was 80°, the blood at the surface was 82°, and a little below the surface 81°. Its smell was offensive and ammoniacal, and its liquidity was increased. On the 23d, the temperature of the blood was the same as that of the water ; and so it continued, whilst slowly undergoing decomposition.

The colouring matter of blood.—About two pounds of the colouring matter of the blood of an ox, from which the fibrin had been separated in the usual way by agitation, but not deprived of serum, was exposed to observation on the 19th of August. It was examined once or twice daily. Its temperature never rose perceptibly above that of the water. On the 22d, it had an offensive smell, and mixed with lime, gave off an ammoniacal odour. In a few days the odour became more strongly ammoniacal and less offensive ; and so it continued afterwards, whilst gradually undergoing the changes peculiar to its very slow decomposition.

Serum of blood.—The observations which I have made on the putrefactive decomposition of this fluid, do not indicate any sensible elevation of temperature attending it : a result, which might be expected, considering that its putrid changes take place very slowly, and that many months are required for its complete decomposition, whether the quantity of serum be large or small, and even when freely exposed to the air in a damp atmosphere.

Fibrin of blood.—On the 19th August, at 4 p. m. the fibrin of the whole blood of an ox was separated in the usual manner, by agitation, and was immediately well-washed and drawn out loosely, so that its surface might be well exposed to the air. In its moist state, as it was placed for observation, it weighed about four ounces and a quarter. On the following day, when the water was 78°, the fibrin about an inch below the surface was 85° ; its odour was peculiar but not fetid ; this was about noon. At 2 p. m. the water being of the same temperature, the fibrin was 90° ; its odour was peculiar and rather offensive. At 6 p. m. the water the same, the

fibrin had fallen to 87°. It had become soft, as it were melting or deliquescing, and in consequence had subsided so much, that it occupied only about half the space it did when first introduced, and of course was much less exposed to the action of the air. It was slightly alkaline, and a little of it, mixed with hydrate of lime, emitted a distinct ammoniacal odour. On the 21st, the water being 79°, the fibrin was only 80°. It was semi-fluid, of a light brown colour, and had a peculiar, rather disagreeable smell, distinctly ammoniacal. On the 23d, its temperature and that of the water were the same; it was of a light gray colour, so fluid as to admit of being poured out in drops, and it had a pretty strong and tolerably pure ammoniacal odour. After this, there was no sensible elevation of temperature attending the changes which slowly took place during its progressive decomposition.

These experiments on the blood and its ingredients, I may remark, are not solitary ones; they have been repeated several times; and the results have been similar; and similar generally, so far as I have observed, whether the subject was the blood of the ox or human blood.

Muscle.—On the 25th August, the psoas muscle of a man who died the same day, and of whose case I shall take brief notice in an appendix, as No. 1, was placed for observation. No change of temperature was observable till the 27th; then it was 2° higher than the water; its odour was not putrid. On the following day, its temperature was the same; its odour peculiar and disagreeable, but not distinctly fetid; its surface was sprinkled with the ova of flies, and with minute larvæ. On the 29th, it was 3° above the water. On the 30th, at the surface, it was 5° higher, whilst about an inch below, its temperature was the same as that of the water. On its surface there was some froth, and larvæ in great abundance. The odour of the surface was peculiar and ammoniacal; of the part below the surface, peculiar, but not distinctly ammoniacal. On the 31st, it was 7° above the water, when larvæ abounded, and were feeding voraciously, and there was a strong ammoniacal odour. The experiment was then interrupted by excluding atmospheric air; the froth subsided, the larvæ died, and the temperature of the part fell to that of the water. A similar experiment was made with the ventricular portion of the heart of the same subject, and with precisely similar results. Other experiments have been made in a somewhat different form, and results have been obtained also somewhat different. When flies have been excluded, muscle has undergone a comparatively slow change; it has gradually softened, become unctuous, acquired a peculiar odour, lost its cohesion, and has become converted into a reddish semi-fluid, not unlike chyme, with a very strong ammoniacal odour. In one instance, this change took place between the 6th and the 21st of August. The temperature was not elevated more than 2° at its maximum. When the muscular fibre was unravelled, and an extensive surface exposed to the air, the change was very much more rapid; the

conversion of muscle into pultaceous fluid took place in six days, and the temperature was elevated 7°.

Brain.—I shall give two different experiments made on this organ entire, or nearly so, deprived of its membranes, belonging to bodies No. 1 and 2. That on No. 2, was commenced on the 14th July. On the 15th, its temperature and that of the water were the same, viz. 76°. On the 16th, at its surface, it had risen 3°; whilst the deeply seated portions varied according to their depth, from 2° to 1°. The surface was then soft, brownish and fetid; what was below the surface was apparently little altered. The temperature of the brain continued increasing till the 21st, when it had risen 11°, then its surface was covered with froth; larvæ of large size were in great plenty, and were feeding greedily; and the odour was strongly ammoniacal. From this time the temperature gradually fell. On the 31st, it was only 2° higher than the water; on the 1st August only 1°; and on the 4th, there was no difference. At this time, its surface was gray, soft, of saponaceous appearance, free from larvæ, and emitting a powerful ammoniacal odour; and even then, two inches below the surface, it appeared to be but little changed. The experiment with brain No. 1, was begun on the 25th July. The highest temperature attained by it was only 1° above that of the water; and on the 11th August, it was the same as that of the water. In this instance, the number of larvæ generated on its surface was less than in the former; their growth was less rapid; they fed with less avidity—the surface was comparatively tranquil, and the ammoniacal odour comparatively faint; and latterly a rancid peculiar odour was rather predominant.

Lung.—It belonged to body No. 1, and was put into the vessel for observation, divided into portions not very small. The highest temperature which it acquired was on the 30th July, then the water being 77°; its temperature, at the surface, and just below the surface, varied from 78° to 90°. Where the low temperature was observable, there it was tranquil and without larvæ; where the temperature was high, there larvæ abounded and were in great activity—the space they occupied was surrounded by a margin of froth, the ammoniacal odour was strong, and the portion was becoming of a blackish hue and liquid. The experiment was then interrupted by the exclusion of air. On the following day the larvæ appeared dead, and the temperature had fallen to that of the water. On the 2d August, atmospheric air was re-admitted. The temperature of the whole, never again exceeded that of the water perceptibly, though it did partially, as in portions where larvæ were collected feeding, and where froth was produced: thus, on the 6th August, whilst the surface in general was covered with a grayish byssus, and was only at the temperature of the water, in one spot about an inch in diameter it was 6° higher, and there in the midst of froth, there was a brood of larvæ rapidly growing and eagerly feeding. On the 27th August, the whole presented the appearance of an inky fluid, of the temperature of the water, and strongly am-

moniacal, apparently deriving its black colour from carbonaceous matter liberated and suspended, mixed with particles of cellular tissue, and at the bottom with shreds of pleura and fragments of vessels, which became apparent on washing.

Liver.—It belonged to body No. 3, and was placed for observation on the 3d August. On the 5th, its temperature was the same as that of the water; its odour was offensive, but not ammoniacal; a good deal of reddish fluid had exuded, and a little air was in the act of disengagement. On the 4th, in some places it was 1° higher, and in others 2°. Where highest, some froth was produced, and many small larvæ were feeding. The temperature gradually increased till the 10th, when it was raised 12°; its surface was covered with larvæ feeding voraciously in froth, and nearly full grown, and the odour was powerfully ammoniacal. From this time it gradually decreased, till, on the 13th, it did not exceed that of the water; most of the larvæ were dead; the odour was very ammoniacal; the surface was free from froth, and of a dark brown colour, in some places almost black and of a soft pultaceous consistence. Beneath the surface the change decreased with the depth; and in the lower part, the liver was comparatively little altered; its parenchymatous substance was softened, of a gray hue, and easily removed by gentle pressure, aided by a stream of water; whilst the vascular structure, (at least all but the very minute branches) retained sufficient strength to resist the pressure and water, and accordingly became well displayed by the operation.

Intestines.—These were from body No. 4, and were placed for observation, after having been washed out, on the 3d of July. Their temperature on the 4th was the same as that of the water, though the odour arising from them was offensive. On the 5th their temperature was 2° higher; on the 6th the same as that of the water; and so it continued, although a great deal of fetid air was disengaged, and the surface had become pultaceous and emitted an ammoniacal odour. The stomach of No. 1 afforded results very similar. Its temperature never exceeded that of the water more than 1°, and that only partially, where a few larvæ were feeding, and a little froth was excited. And, in both instances, below the surface two or three inches, there was little change. After a month, the parts appeared entire; but, on minute examination, after having been well washed, the tissue which remained seemed to be chiefly the submucous cellular and the peritoneal; neither the inner coat, nor the muscular, could be recognized; and even the white tissues were, in part, losing their cohesion, and exhibited evident marks of slow decomposition being in progress.

Cutis of inside of thigh, dura mater, tendo achillis.—These parts were taken from body No. 1, and were placed together in the same vessel for observation. They underwent change slowly; their temperature gradually rose till it was 3° above that of the water, viz.—on the 31st July, when the odour from them was

slightly ammoniacal, their surface unctuous, and there were a few larvæ feeding. After the 2d August, the difference of temperature was 1°, and it was the same on the 15th of that month. A good deal of yellowish pultaceous matter was formed, by which the parts were hid. It was washed off, and the different textures were found nearly equally changed; their cohesion irregularly diminished; the fine connecting tissue, or most delicate fibre, dissolved entirely or in part; holes formed in the two first mentioned textures, and the coarser fibres, or fascienli of fibres, separated in the last. On the 1st September they were all reduced to an ammoniacal pultaceous state, in which only a few minute tendinous fibres were distinguishable.

Aorta.—A portion of this vessel, belonging to body No. 1, placed for observation, was 1° higher than the water on the 30th July. It had a peculiar and ammoniacal odour, and its surface was becoming unctuous, especially its compact fibrous sheath, or the third coat from the inner one. On the 11th August there was an elevation of 3°; a good deal of light brown pultaceous matter had formed; the odour was strongly ammoniacal, and many larvæ were feeding. On the 16th August, it was entirely converted into a semi-fluid opaque mass, of a light brown colour, and strong ammoniacal odour, and its temperature was the same as that of the water. I have also made trial of the different coats of the aorta, placed apart for observation, viz.—the loose outer cellular, containing some fatty matter—the adjoining compact cellular, or rather fibrous—the next commonly called the middle coat, and the inner coat—taken from body No. 5. The changes in all of them proceeded much in the same manner as has been described in the preceding instance, with the exception of the loose cellular coat, which, in that instance, was excluded; it did not keep progress with the rest—after a month's exposure it was very little changed.

Intervertebral substance.—A portion from the lumbar spine was taken from body No. 5, on the 7th of August. On the 13th, it had a peculiar odour, neither putrid nor ammoniacal. After a month, it was very little changed; and, during that time, its temperature was not sensibly elevated above that of the water. From other experiments, I have ascertained, that a very long time is required for the decomposition of this substance, and that even after a year, it is not very considerably changed.

Besides the preceding, I have made trial of other parts, as the pancreas, kidney, spleen, prostate. They have undergone change rapidly like the liver; their temperature has been considerably elevated; they have all softened, as it were liquefied like fibrin, and ammonia in great quantity has been generated.

I apprehend, more than sufficient has been adduced, in the results of the preceding observations, to decide the question proposed —" Whether or no the putrefaction of animal matter is attended with elevation of temperature?" It seems proved that those parts which undergo rapid change, as the fibrin of the blood, brain, mus-

cular fibre, and the parenchymatous substance of the glandular structures, give off a good deal of heat in being converted from the solid into the liquid form : and, as other parts undergo a similar change, and are ultimately reduced to a liquid pultaceous state, and as, in some of them, there is a small elevation of temperature, may it not be fairly inferred, that the apparently inferior degree of heat generated by them, is merely owing to slowness of change : and, accordingly, whatever accelerates the process of decomposition, seems to promote increase of temperature. Minute mechanical division has this effect in a remarkable manner, as was witnessed in the instance of the fibrin of the blood, and of muscular fibre ; the effect is probably owing to the very extensive surface exposed to the action of atmospheric air.   The presence of larvæ too, has the same effect very strikingly, and probably in the same way.   They tend to separate the parts, to agitate the surface, and to introduce air beneath the surface.   It is curious to watch the progress of these animals in their growth, and still more so in their operations. When their food is very nutritious, the almost microscopic ova, in forty-six hours are converted into large maggots. When they have nearly attained their full size, they feed with extraordinary voracity, as if aware that their lives depended upon their activity. The whole of a numerous brood might be observed, side by side erect, with one extremity in the ammoniacal pultaceous mass, pumping up nourishment, and with the other and narrower extremity in the atmosphere, the orifice of its canal dilated, seeming to pump down air ; and in the act, by the difference of the specific gravities of the two fluids—it may be inferred that the perpendicular position of the larvæ is maintained, whilst probably by the action of one on the other, a compound is formed fit for assimilation and for forming a part of the new animal, the putrefactive process at the same time being accelerated. It appeared probable at first view, that the larvæ themselves might generate and possess a comparatively high temperature ; but, on the whole, the observations which I made to endeavour to determine this point, are not in favour of the notion. When the larvæ are distended with putrid matter, exhaling ammonia and rapidly undergoing change, then their temperature, as might be expected, is always above that of the atmosphere ; but, when they are free from putrid matter internally and externally, although collected together in a large number, in a small space, as in a phial, a delicate thermometer placed in the midst of them, did not indicate the generation of any sensible degree of heat.   And, in conformity with this, I found that the larvæ by themselves viti-ated the air very slowly ; indeed, I believe that their dead bodies vitiate it more rapidly than their living.   The changes in the dead bodies, in putrefying and becoming semi-fluid, accompanied by the formation of carbonic acid and ammonia, are more rapid than the changes in their living bodies in passing from the larvæ through the chrysalis state to that of the imago, or in becoming as it were solid, which change seems to be accompanied chiefly by the exha-

lation of water, and the formation of a very little carbonic acid gas. Mixture probably is another circumstance which favours and accelerates the putrefactive process and the evolution of heat. The two circumstances already adduced may partly act in this manner, as well as by exposing a greater surface to the air. In the saccharine or vinous fermentation, the effect of mixture is well marked. So long as the sweet vegetable juice is retained in the tubes or cells of the plant or fruit, it resists change ; but, the instant the tubes are crushed, and the cells broken down, the fermentation under favourable circumstances commences. I suspect something similar to this happens with animal matter. It is known to every one that bruised meat putrefies very rapidly. An egg, carefully put by, exposed freely to the atmosphere, without any covering over its shell, will remain free from putridity for months ; and in a dry atmosphere, will lose the greater proportion of the water which it contains, and on breaking it, the yolk and white will be found dry and contracted. But, if the yolk and white are intimately mixed by violent agitation, then, I believe, the mixture, even in the shell, will soon become putrid. Certain I am that when freely exposed to the air, out of the shell, the yolk being merely immersed in the white, they do not putrefy, though, if the two are mixed, in a short time they undergo a change ; they become highly putrid, and the temperature of the mixture rises several degrees.

4.—Some general remarks on the putrefactive process.

What is animal putrefaction, or the putrid fermentation ? I am not aware that this question has yet been replied to, in a satisfactory manner, or that the present state of our information is such as to admit of making the reply free from many obscurities. The little information I have collected bearing on the subject, I shall bring forward without hesitation. Under diminished pressure, conducted in tubes over mercury, the putrid fermentation appears in some of its changes analogous to the vinous. The elements of vinous fermentation appear to be leaven, which remains unaltered,[1] and sugar which undergoes change ; and the helping circumstances, a certain degree of dilution with water, and a certain temperature. The elements of the putrid fermentation are less distinct. If there be, as is probable, an animal leaven, it has not yet been isolated. Whether the presence of oxygen is essential to the commencement of the vinous fermentation is matter of doubt. But the presence of oxygen

[1] The same portion of leaven may be repeatedly used. I have preserved the leaven obtained from the grape for nearly twelve months, by pouring off the fermented fluid, as soon as the leaven had subsided, and adding fresh sugar and water. How leaven acts is one of the mysteries of chemistry, and there are others of a like kind—as the influence of carbonate of lime in the formation of saltpetre—of sulphuric acid in the formation of sugar from starch, &c.

seems to be undoubtedly necessary to the commencement of the putrid fermentation, and only to its commencement, as we have seen that it goes on over mercury, after atmospheric air has been entirely excluded.' The products of the vinous fermentation in the simplest form, appear to be very few. Alcohol is the only liquid product known—and carbonic acid gas I believe is the only essential gaseous product. The disengagement of azote, probably, when it occurs, is accidental; may it not be referred in the early stage of the process to the atmospheric air entangled in the materials, and in the advanced stage to the decomposition of a little leaven? I have made many experiments on this subject, which have led me to this conclusion. The products of the putrid fermentation seem to be more numerous. Carbonic acid gas is the principal gaseous one. Sulphuretted hydrogen, and carburetted hydrogen, may be occasionally detected. Azote, I believe, is not disengaged, excepting at a very advanced period, when, probably, the products themselves are decomposing. The products not evolved in the gaseous state appear to be chiefly ammonia, kept down in confinement a while by carbonic acid and water; and a substance, or matter in a semi-fluid form which has a considerable resemblance to chyme, and which appears to be the proper food of the larvæ of many different kinds of the extensive genus Musca—some preferring the matter from muscle, some from tendon, &c. And this matter itself seems to undergo decomposition, and to be converted into ammonia, and a substance more resembling extractive matter after it has been exposed to the air, than any other with which I am acquainted, excepting one, procurable by the spontaneous decomposition of some kinds of vinegar to which atmospheric air is allowed to have access. During the decomposition of the vinegar, carbonic acid is formed by the union of carbon with oxygen. The vinegar gradually loses its acidity, and at the same time a pellicle forms on its surface, and gradually increasing, becomes a thick, strong, tough, transparent membrane—elastic, strong, and transparent when dry, insoluble in water, and having great power of resisting change, whether acted on by heat, by acids, or alkalies. Very analogous to this, is the substance alluded to, which I have witnessed on the surface of serum and fibrin, and of several solid parts in water in experiments on their putrefaction, continued from six to twelve months. The product from vinegar does not contain azote; that from the animal

¹ From experiments already related, it appears that venous blood as well as arterial, contains oxygen in a peculiar state of combination associated with the red particles: this oxygen may be sufficient to excite putrefaction after death, in every part of the body penetrated by the blood independent of the atmosphere; and to account for the very rapid manner in which bodies putrefy, if abounding in blood, as is often the case in instances of sudden death, and for their putrefying under water in instances of death from drowning. The very tympanitic state which bodies acquire so circumstanced—indicates, that their different textures, especially the integuments, are little pervious to air—which is also in accordance with the results of Priestley's observations on the power of bladder when thoroughly wet to confine air.

fluids and textures has afforded ammonia when decomposed with lime, at a red heat; but, as I am not sure that any specimen of it which I have yet examined has been perfectly pure, I cannot be certain that this difference of composition really exists between the two.

Under ordinary atmospheric pressure and exposure to the atmosphere, the results of the putrefactive fermentation are various and modified; the circumstances become complicated, and the difficulty of appreciating them is vastly increased. The gases generated and absorbed, in part seem to react, and to exert a powerful modifying influence, both as retarding putrefaction and giving rise to combinations which have a similar tendency; and inspissation at the surface from evaporation, by forming a crust, more or less impervious to the air generated beneath, as well as to the external air, has a similar effect. Carbonic acid gas is powerfully antiseptic—so are ammonia, and carbonate of ammonia, and sulphuretted hydrogen, as I have ascertained by experiments made expressly for the purpose. Thus, blood saturated with carbonic acid gas, did not putrefy so soon, as another portion to which none of the acid gas was added. A solution of the sesquicarbonate of ammonia, in the proportion of three grains to the ounce of water, prevented the putrefaction of a portion of muscle immersed in it about a month; and a solution of the strength of five grains to the ounce of water, preserved a piece of jejunum more than eight months, and I know not how much longer; and displayed the minute structure of the part, in a very striking manner. Reduction of temperature is another disturbing circumstance, which tends, I believe, to give rise to new combinations, especially to the formation of adipocire, and to render the problem as to the changes which take place more complicated.[1] Even in this view of the subject, there is no want of similarity between the putrid fermentation and the vinous; and a considerable degree of analogy might be traced, very far, were we to consider the particular agents which retard or prevent the two kinds of fermentation; indeed, I am not acquainted with a single agent, or circumstance, the effect of which is limited to the vinous, or to the putrefactive fermentation. They all appear common to both, and also to the fermentation of vegetable matter generally. Such is the effect of corrosive sublimate, of the sulphurous acid, of tannin, of salt, vinegar, alum, spirit of wine, sugar, &c. These circumstances, just related, prove some resemblance between the two kinds of fermentation, in their general phenomena —in the causes which promote and accelerate, and those which retard and prevent them. The leaven which excites vinous fermentation, closely resembles an animal substance in composition and in many of its properties, and is even analogous to fibrin. Like

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[1] Adipocire, I believe, like peat, never forms where the atmospheric temperature is high; I have never witnessed the production of either of them in warm climates, excepting, in the instance of peat, on mountainous regions, where the average temperature is moderate, and in that of adipocire, during the cool months, in the Mediterranean.

fibrin, it is capable of undergoing the putrid fermentation;[1] and the
fibrin and muscular fibre act as leaven with sugar and water, and
give rise to fermentation.   Whether the fermentation, thus excited,
is similar or not to the ordinary vinous fermentation remains to be
shown.   I suspect it is not similar in all its products.   Carbonic
acid gas is disengaged; the muscle or fibrin is prevented from putre-
fying—and if tainted, is corrected; but, I am not aware that alcohol
is formed.   In one instance, the sugar, by the fermentation, appeared
to be converted into gum, powerfully absorbent in water—but very
slightly soluble in water.   It was the result of three or four days
fermentation of a solution of refined sugar in water, excited by a
small portion of beef.   It appears too, not improbable, that a prin-
ciple may exist, contained in the fibrin of the blood, in muscular
fibre, and in the parenchymatous substance of the very putrescible
organs in large proportion, which may act as leaven in animal com-
pounds; and I might mention some facts in support of the notion,
were I not apprehensive of extending this part of the subject beyond
its due limits, and the more so, as I wish briefly to advert to one or
two other points of considerable theoretical interest.

After witnessing the change which takes place in muscular fibre,
in consequence of putrefaction giving rise to a fluid very like chyme
in appearance, I was disposed to ask, may it not be concerned in
digestion itself, according to the earliest theoretical notions on this
subject? and also, after observing the effects of the putrefactive pro-
cess in raising the temperature, inquire, may it not in the stomach
and intestines, be one of the sources of animal heat?

Meat slightly putrid, we know, is highly nutritive, and very easily
digested.   When meat is becoming tender by keeping, it is in an
incipient state of putrefaction; the muscular fibre is losing its cohe-
sion; the textures generally in a very minor degree are doing the
same, and carbonic acid gas is forming, as I have ascertained by
experiment.   Now, as we know, that carbonic acid gas is formed
in, or disengaged from the stomach and intestines, even when the
function of digestion is apparently in its healthy state, it does not
seem unreasonable to consider this gas a product of digestion, almost
as much as of fermentation.   The other gases, as sulphuretted hy-
drogen, and carburetted hydrogen, occasionally evolved during the
putrid changes, are frequently generated in the alimentary canal;
and, the contents of the stomach, and of the whole intestines are
seldom, I believe, destitute of ammonia.   In relation to this last
mentioned product of the putrid fermentation, I may mention, that
whenever I have sought for it after death, either in the human
stomach, or in the duodenum, jejunum, or ileum, I have always

[1] It also promotes destructive decomposition in vegetable matters in them-
selves little prone to change.   I remember witnessing this effect in a striking
manner in a piece of fine table-linen which was sent to the Mediterranean,
with other things, and amongst them a bottle of yeast, which burst on the
voyage; the damask was to a considerable extent destroyed, but only where
the yeast had come in contact with it.

found it; and I need not add, that I have always found it in the large intestines, and in instances in which the *post mortem* examination was made before there were any indications of putrefaction having commenced in the solids or fluids generally, and when ammonia could not be detected either in the blood,[1] the bile, the brain, or the muscles. The result of the examination of animals just killed, and which I have made in relation to this point, has not been less conclusive. In one instance, I had a hungry dog, fed at three different times during the twenty-four hours, with fresh beef. Three hours after the last feeding, he was killed. All the meat given him had disappeared, excepting the last meal; and the portions of beef of which it consisted, were very little altered: yet, triturated with hydrate of lime, they gave off a distinct ammoniacal odour—so did a little whitish chyme in the pyloric extremity of the stomach; the chyme in the duodenum gave it off more strongly; a thin yellowish fluid in the ileum not less so; and the contents of the large intestines in a hardly less degree. I examined the blood, both venous and arterial, the chyle, the bile, and several of the solid parts; but none of them afforded, by the test of lime, the slightest trace of ammonia, not even the kidneys, although the urine abounded in ammoniacal salts, and emitted on the addition of lime a most powerful ammoniacal odour. These circumstances seem in harmony with the old hypothesis—that putrefaction is concerned in digestion.

In opposition, many facts may be stated, tending to show, that digestion is a specific action, or series of actions—that its phenomena are vital, or in other words, peculiar, mysterious, and inexplicable according to the present state of our knowledge. Many of the results of the experiments of Spallanzani, and Stephens, are of this character. Contrary to the idea of putrefaction, the contents of the stomach are generally acrid; and the acidity diminishes almost gradually from the stomach to the rectum. Ammoniacal salts do indeed exist in the stomach and intestines; but in a healthy state, I am not aware, that ammonia, or the carbonate of ammonia, has ever been detected in these organs; and consequently, the principal product of putrefaction is found wanting.[2] Moreover, putridity seems

[1] In the trials referred to above, the blood and other parts were mixed with quicklime, and the absence of ammonia was inferred, if no ammoniacal odour was perceptible. Recently, I have had recourse to a severer test of the volatile alkali, viz. bringing near the mixture under trial, a glass rod moistened with muriatic acid, so diluted, that no fumes were produced, merely by aqueous vapour. Using this means, in every instance, I have detected ammonia in the blood, even when it has been extracted from the body very soon after death. The degree of effect, however, has very much varied; in some cases, in which the blood-particles appeared under the microscope very little, if at all altered—the indications of ammonia afforded were only just perceptible: on the contrary, when the colouring matter of the blood was partially dissolved in the serum, and the forms of the blood-particles were altered, then the indications of ammonia were strong.

[2] This, it may be said, is owing to the presence of a stronger acid, such as the muriatic, which seems to be, from the experiments of Dr. Prout, an

to be corrected in the stomach, and its natural progress stopped. I shall not lay stress on meat introduced in a putrid state into the stomach, having been taken out after a time free from taint, as it may be said, that the putrid particles may have been destroyed, modified, or removed, as happens when tainted meat is placed in a saccharine fluid in a state of fermentation.   I would rather adduce the fact, which I have witnessed, that muscle which has been in the stomach some time, when taken out and exposed to the atmosphere, does not putrefy more rapidly, but in a remarkable manner resists putrefaction. The substance or matter derived from muscular fibre, undergoing the putrefactive change, is indeed not unlike chyme in appearance, whether viewed with the naked eye, or with the microscope—both of them abounding in minute particles and containing some globules.   But of how little value is such a remark—and how little to be insisted on !   Many different semi-fluids might be mentioned bearing as close a resemblance.   It is more to the purpose to compare the matter resulting from putrefaction with chyle.   How great is the difference of the two!   The chyle is alkaline, and yet contains no sensible quantity of ammonia; there is no reason to suppose that it contains any free carbonic acid; it seems as if it were altogether a new formation. Chyle derived from the muscular fibre of the ox, in the thoracic duct of the dog, or that of the ox derived from grass, consists of a milk-like serum, coagulable by acids, and of lymph spontaneously coagulable, resembling the fibrin of the blood in its manner of coagulating, and gradually contracting, and resembling it also in its manner of undergoing the putrid decomposition on exposure to the air by deliquescing, as it were, or becoming semi-fluid.   I will not say how incompatible all this appears to be with putrefaction, but more, how inexplicable in relation to our imperfect knowledge of the chemical changes taking place amongst the elements of matter destitute of life, or beyond the influence of the vital organs.

If putrefaction is not admitted to be concerned in digestion, the second question asked above is answered as regards it—" Whether in the stomach and intestines it may not be a source of animal heat? But substituting digestion for putrefaction, an answer still may be required.   The circumstance that some carbonic acid gas is formed in the stomach and intestines, (supposing that which is found, is formed there,) is favourable to the idea of digestion being an auxiliary source of heat.   The observations which I have made on the temperature of the stomach in animals, is also in favour of the idea: I have often found it as high as that of the heart, and sometimes. though rarely, a degree higher.   The conversion of a solid into a fluid, as of solid food into chyme, or chyle, appears rather an objection to the notion.   But, I am not sure that is a valid objection, when properly considered, there being so many remarkable excep-

active agent in digestion, and which of course, if free, would unite with any free ammonia formed; so far weakening the above argument.

tions of the kind, even in instances in which the change of form is greater, as in that of the conversion of a solid into a gas, accompanied by combustion, without condensation, even of the supporter of combustion; and *vice versa*, of the formation of a solid from the union of two gases without change of temperature. Neither am I aware of any remarkable change of form of an animal or vegetable substance, which is accompanied either by absorption, or by evolution of heat. The fibrin of the blood appears to coagulate without change of temperature; no heat is evolved when serum is coagulated by an acid; none when a strong solution of gum is coagulated by sugar of lead; none when milk is coagulated by rennet.

The influence of the various secreting organs on the animal temperature is, no doubt, highly deserving of inquiry and of experimental investigation. Were I to draw an inference further from the observations which I have made on the temperature of different parts of the body, I should be disposed to say, that besides the primæ viæ, there is no other apparent auxiliary source of heat, (considering the lungs as the main source)—neither the brain, the temperature of which is comparatively low, nor the kidneys, the fluid secreted by which appears in the bladder to be of rather lower temperature than the blood in the heart and great vessels; nor the liver, the temperature of which I have never seen above that degree which might be expected, taking into account its proximity to the lungs, its bulk and the abundant manner in which it is supplied with blood.

In a former part of this work "on the temperature of the human body after death," two instances are given of an extraordinary high degree of temperature observed in bodies which had been dead a short time; and in both of which the fibrin of the blood had disappeared, as it does in putrefying, and yet the bodies exhibited none of the usual signs of the putrid change having even sensibly commenced. I then concluded in consequence, that the unusual temperature was not owing to putrefaction—or a *post mortem* effect; and, reflecting on all the circumstances of these cases, and on the phenomena witnessed in my experiments on putrefaction, I am confirmed in that inference, and obliged to suppose that it either arose from the ordinary sources of animal heat being more energetic than usual, or from the cooling process being comparatively inefficient, or else to imagine some peculiar morbid change in the blood itself, on which, in common with the disappearance of the fibrin, the unusual temperature may have depended. In all the instances recorded in the preceding pages, in which, during putrefaction there was a considerable elevation of temperature, it was at a particular period of the process, namely, when it was most active, when the greatest apparent change occurred, before the oxygen of the atmospheric air had disappeared, and when the change was attended with the formation of carbonic acid gas, partly at least, by the union of the oxygen of the air, with the carbon of the putrefying matter.

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Whether any heat was produced during the after part of the pro-
cess, when the changes were taking place more slowly, when oxy-
gen was excluded, and carbonic acid gas was formed from its ele-
ments in the compound in the same manner as ammonia was
formed, may be matter of question. The results themselves were
far from conclusive—rather hostile than favourable to the idea of
heat being thus generated. The analogy of vinous fermentation,
in which there is a manifest elevation of temperature produced by
the arrangements of the elements in new combinations, independent
of the presence of oxygen, may be adduced in support of the notion.
Were the proportion of carbonic acid gas generated in the two kinds
of fermentation similar, then the analogy would be strong; but, in
the putrid fermentation, the quantity of carbonic acid gas formed,
independent of the oxygen of atmospheric air, is small, and the
quantity of ammonia large. If the formation of ammonia, like that
of carbonic acid gas, were usually attended with increase of tem-
perature, then there would be reason to suppose that the fermenting
process of animal matter throughout is a heating one. But, I know
no proof, that heat is evolved in the union of the elements to form
ammonia. In relation to heat, it may be remarked, that azote and
ammonia, and the combinations of the latter, and of most of the
former, are peculiar and anomalous. Elastic fluids are formed by
the sudden decomposition of solid and liquid bodies (the detonating
compounds) with little change of temperature—and *that* an eleva-
tion, not a diminution of temperature. Ammoniacal gas unites
respectively with the carbonic, muriatic, and fluo-boracic acid gases,
forming solid salts, without the production of heat; and as during
the slow decomposition of ammonia by electricity, there is no evi-
dence of change of temperature, so equally there is want of evidence
of the same in its formation. The probability, therefore, is, that its
elements unite without evolution or generation of heat; and conse-
quently in the putrid fermentation it cannot be considered as a
cause of heat.

Appendix,—containing a brief notice of the fatal cases referred to.

No. 1.—Aged 42, died on 25th July, of an abscess in the liver.
The body was examined ten hours after death. It was much ema-
ciated; excepting the liver, the lower part of the ileum, and the
upper part of the colon, the parts generally were sound. The por-
tions of the intestines alluded to, were slightly ulcerated.

2.—Aged 26; very little emaciated; died on the 14th July, of
acute dysentery. The body was examined six hours after death.
All the organs were tolerably sound excepting the large intestines,
which exhibited the worst effects of the disease mentioned.

3.—Aged 23; not much emaciated; died on the 2d August, of
acute dysentery. The examination was made nineteen hours after
death. The pericardium adhered to the heart; there was an abscess
in the liver contiguous to the gall-bladder; the inner surface of the

gall-bladder was very red, and it contained a serous fluid; the inner coat of the colon was severely ulcerated; the other organs were tolerably sound.

4.—Aged 35; rather emaciated; lower extremities œdematous; died on the 3d July, of disease of the heart and brain. The examination was made ten hours and a half after death. Both sides of the heart were much thickened, and their cavities enlarged; the central parts of the cerebrum were very much softened; the other organs, with slight exceptions, were tolerably sound.

5.—Aged 26; rather emaciated; died on the 7th August, of acute dysentery. The examination was made six hours after death. No lesion was discovered worthy of notice, excepting in the large intestines, which were very extensively ulcerated, &c.

XV.

EXPERIMENTS AND OBSERVATIONS ON THE MACERATION IN WATER OF DIFFERENT TEXTURES OF THE HUMAN BODY.

Although the process of maceration by means of water has been long in use amongst anatomists in making preparations of particular parts, especially of the more solid; and though, of late years, many observations have been collected bearing on this operation, yet I am not aware that any inquirer has yet engaged in the subject systematically,—has attempted to investigate it through the changes which it produces, or even to ascertain with any accuracy what changes really are produced. Nor is this neglect surprising, considering the circumstances attending the maceration of animal matter,—the difficulty of procuring subjects,—the conveniences requisite,—the time required,—the great attention necessary to observe the fleeting phenomena,—the disgusting nature of the experiments, —and the idea of danger of health attached to them.

It was when stationed in Malta, in the year 1828, that I was induced to engage in the investigation for the purpose of removing doubts, and of gaining information which was not to be collected from books. The results then obtained, were published in the 41st volume of the Edinburgh Medical and Surgical Journal; though they were far from being so finished as I could wish, I again bring them forward with the hope, that as a contribution to anatomy and animal chemistry, they may be of use.

1.— *The circumstances attending the experiments.*

The experiments were all made in the laboratory of the general

hospital,—formerly the hospital of the knights,—a room, by its equable temperature, well adapted for the purpose, and they were carried on during a period very nearly comprehending twelve months. The water used for maceration was aqueduct water, that of the town of Valetta, which appears to be very much the same in quality throughout the year; and seems to be variable chiefly in temperature, from being conveyed a distance of about eight miles, and a considerable part of the way, running very slightly protected from atmospheric influence. During the spring months its temperature varied from about 55° of Fahrenheit to 65°; during the summer from about 65° to 75°; during the autumnal from about 75° to 65°; and during the winter from about 65° to 55°. In the cool season it was always a few degrees above the temperature of the room, and in the hot season a few degrees below it. But as the water was changed every day, or every second day at furthest, and as a gallon or two was generally used for maceration, the alteration of the temperature of the water during the progress of the experiments was immaterial and undeserving of particular attention. I have said that the quality of the water was apparently always much the same. This I have not proved by analysis: it is conjectured from considering that the aqueduct is fed by springs in limestone rock which are perennial, and very little affected by the seasons. In July, just before I entered on the inquiry, I particularly examined the water. Its temperature then as it flowed from the pipe into the room, was 73°, its specific gravity was 1008; 100 measures of it contained 4 measures of air, and this air consisted of 10 carbonic acid gas, 62 azote, 28 oxygen.

Its saline and earthy contents were very inconsiderable, not quite $\frac{1}{5000}$ part, and consisted chiefly of carbonate of lime, dissolved by carbonic acid, and of common salt, with a very little lime and magnesia, and fixed alkali in combination with the sulphuric and muriatic acids. The vessels in which the parts were subjected to maceration were glass jars, varying in capacity from one to two gallons. In some instances they were covered slightly to keep out dust; in others they were uncovered. The subjects of the experiments were generally suspended in the water by common pack thread, and to prevent their floating when air was generated, they were kept under by a counterpoise of glass attached by the same kind of thread to their inferior surface.

2.—A brief account of the bodies from which the parts, the subjects of the experiments, were taken.

It may be remarked generally, that all the bodies were of the male sex; and all of them, with the exception of two, of soldiers of our regiments.

1.—Aged 25; died unexpectedly on the 24th August, 1828, labouring under symptoms of continued fever of five days' duration. The body was examined about five hours after death. Nothing satisfactory was discovered to account for the fatal event. Blood, in

large quantity, had been abstracted twice, and active purges had been used.

2.—Aged 29; died on the 10th October, 1828.   The body was examined twenty-two hours after.   It was much emaciated; the cerebrum was in a very soft state, especially its central parts; the lungs abounded in minute tubercles; the superior lobe of the right lung contained a tubercular excavation; and the lower part of the ileum was slightly ulcerated.

3.—Aged 23; died on the 16th October, 1828, of "continued fever" (as the disease was called) of nine days' duration, with some anomalous symptoms.   The body was examined fourteen hours after.   The cause of death was not ascertained in a satisfactory manner.   Some lymph was found effused in the posterior mediastinum, and the lungs contiguous to the mediastinum were turgid with blood, and in a certain degree hepatised; the spleen was unusually large and soft; and the lower part of the ileum was slightly and obscurely diseased.

4.—Aged 24, died on the 4th November, 1829, having had the thigh amputated on the 16th October, and the femoral artery tied just below the arteria profunda on the 22nd, on account of secondary hemorrhage.   The body was examined twelve hours after death. The crural, and iliac veins of the diseased limb were thickened by deposition of coagulable lymph.   The lower half of the thigh bone, which had been amputated, was in a very diseased state, as were also the tendons and muscles attached, and the whole of the knee, almost every part of which was altered; whilst the leg and foot, with the exception of œdema, appeared to be in a sound condition.

5.—Aged 56, died suddenly on the 11th November, 1828.   The body was examined fifteen hours after.   The heart was unusually large; the semilunar valves of the aorta were partially ossified; the orifices of the coronary arteries were very much contracted; the arteria innominata, and the arch of the aorta were enlarged; the aorta was generally diseased; its inner coat irregularly thickened, and its middle coat corresponding, more or less absorbed.

6.—Aged 18, died on the 12th November, of dysentery.   The body was examined twelve hours after.   It was considerably emaciated; and the colon exhibited the appearances usual in this disease in its severe chronic state.

7.—Aged 23, died on the 29th of November, of acute dysentery. The body was examined five hours after.   The whole of the left lung, and the superior lobe of the right, abounded in small tubercles; and in the former were two or three vomicæ.   There was a small ulcer in the duodenum.   There were several ulcers in the jejunum, and numerous ones in the ileum and colon.

9.—Aged 32, died on the 26th December.   The body was examined twenty-two hours after.   The liver contained two abscesses. The large intestines were severely ulcerated.

10.—Aged 29, died on the 7th January, of acute dysentery.   The

body was examined sixteen hours after. There were patches of
coagulable lymph on the lower part of the ileum (its villous coat.)
The colon throughout exhibited the worst effects of acute dysentery.

11.—Aged 20, died on the 7th of August, 1829, of acute dysen-
tery. The body was examined six hours after. The colon was very
extensively and severely ulcerated, &c.

12.—Died on the 8th August, of dysentery. The body was exam-
ined nine hours after. The lower part of the ileum was slightly ulcer-
ated. The colon was extensively ulcerated, &c.

3.— *General results of the experiments on the maceration of the different parts of the body.*

The experiments on maceration were commenced immediately
after the inspection of the bodies. The water, as already remarked,
was changed every day or every second day, and the changes pro-
duced were as frequently observed during the first four or six
months, and were noted down from time to time.

Were I to give a minute account of the observations which I have
made on the different parts of the body, whilst undergoing macera-
tion in water, a volume would be required to contain them, the de-
tails would be extremely tedious, and their utility very doubtful. I
shall endeavour to avoid equally the two extremes of great minute-
ness of description and of excessive brevity, and confine myself as
much as possible to a narration of the more important results. I
shall commence with those parts in which the changes produced by
maceration are most rapid, and proceed to those in which they are
least so, but not confining myself to this order, when a natural con-
nection of the parts, or any other reason, renders a deviation from it
useful.

Fibrinous concretion from the heart. The water having been
changed daily from the 4th November to the 12th, the concretion
had become quite white, rather soft, and of a slightly putrid odour.
On the 22nd it was reduced to a kind of pulverulent sediment. The
same change takes place in common fibrin of the blood, whether
separated from the colouring matter artificially or naturally, as in the
buffy coat, and as in the instance above given of the fibrinous con-
cretions formed in the heart. The time required for the alteration
is variable according to the degree of firmness of the fibrin, and ac-
cording to the temperature, being directly as the one, and nearly
inversely, as the other; or in other words, the firmer it is, so much
the longer time is required; and the warmer the weather, or the
water, so much the shorter time.

Muscle—heart. (4. 8.)—Its muscular substance underwent rapid
change; it very soon became putrid and soft, and was washed
away in the form of pultaceous matter, very like that from the
fibrin of the blood; whilst its enveloping membrane, its valves,
tendinous chords, and the great vessels taking their rise from
it, were but little altered. With care at a particular period of

the process of maceration, it was easy to show that the valves of the aorta and pulmonary artery are connected with the outer fibrinous sheath of these vessels; and I am disposed to believe that these valves consist of this tissue in greater proportion, than of the inner coat, and that they owe their strength and peculiar properties rather to the former than to the latter. It is worthy of remark, that the thin muscular substance of the auricles underwent change less rapidly than that of the ventricles. Other muscular parts of the body were subjected to maceration; some of them, as the substance of the tongue, and most of the voluntary muscles exhibited rapid change, like the ventricles of the heart; others, as the muscular coat of the primæ viæ, and especially of the rectum, and of the urinary bladder, more resembled the auricles of the heart in the slow manner in which they underwent alteration.

Brain. (1.)—This organ rapidly becomes soft in water, and forms with it a kind of emulsion, the consistence of which varies of course with the proportion of each. In the state of emulsion it has a peculiar and disagreeable odour, but neither fetid nor ammoniacal. As the water cannot be changed, I do not consider it necessary to give here any particulars of the alterations which take place in the mixture left undisturbed for a considerable time.

Spleen. (6. 7. 8. 10.)—It became soft very rapidly according to circumstances of temperature, &c. in a space of time varying from twenty-four hours to three or four days. When an incision was made into it and gentle pressure applied under water, the softened matter (very like that from fibrin, coloured by the colouring matter of blood) flowed out, fetid, and mixed with some air bubbles; and when it was softened throughout its mass, the operation of pressing it gently, being continued under water, the whole of the softened matter was expressed, and nothing distinctly remained but the vascular structure of the spleen, contained in its enveloping membrane, and presenting a beautiful and instructive appearance, the vessels anastomosing very freely, and in a certain regular manner, and retaining their strength very little impaired. Here I may remark, that with the spleen thus prepared, I have often endeavoured to discover the cribriform structure, of late years attributed by some anatomists to the splenic veins. No where could I observe this structure; every opening in the sides of the veins which I could detect was an opening into a branch. After the separation of the soft matter, when the maceration of the spleen was continued, it slowly underwent change, and wasted away; the more delicate structure disappearing first, and the trunks of the vessel, and the including membrane enduring longest, much in the same manner as the larger vessels generally, and the fibrous and serous membranes, which will be considered hereafter.

Pancreas. (2. 3. 7.)—This organ softened nearly as rapidly as the spleen. When gently pressed under water, the soft fetid matter was separated; and after two or three days, successively renewing the pressure, nothing but its vascular structure and its duct remained,

which latter was of great strength, and resisted further maceration for a long time.

Parotid gland. (6. 9.)—It softened much in the same manner as the pancreas, and the pultaceous matter derived from it was very similar. After the separation of this, there remained a tissue of strong cellular texture, of vessels, nerves, and the branches of the duct, which, the maceration being continued, gradually softened and were very slowly decomposed. The duct itself resisted decomposition very powerfully, and so did a portion of the lining membrane of the mouth, in which it terminated, and from which in minute structure it was not distinguishable.

Thyroid gland. (8.)—It underwent the same changes as the parotid, and in about the same time. When the pultaceous matter generated was expressed, there remained also a tissue of nerves, and cellular texture.

Prostate gland. (12.)—This organ changed rapidly, and was soon reduced to a soft pultaceous mass of a fawn colour, in which a vestige only of cellular tissue could be detected, and the remains of some small vessels.

Kidney. (4.)—It gave off a good deal of the colouring matter of the blood, and was soon reduced to a soft mass. After gentle pressure under water, there remained only the infundibula which had a delicate vascular structure, a few large vessels, its enveloping coat, and the lining membrane of the pelvis, which did not resist prolonged maceration for so long a period as the vessels and membranes of the spleen.

Liver. (3. 4. 8. 9.)—It underwent change pretty rapidly, and in a manner not unlike the kidney. The water was first discoloured by blood, next by gray pultaceous matter derived from the softening of the substance of the viscus. After maceration for a month or six weeks, there remained little besides its vessels, and the branches of its duct. These resisted decomposition much longer, especially the larger vessels, and the hepatic duct, and some of its branches, to which adhered, for a considerable time, very minute grayish or brownish granules.

Lungs. (3. 4.)—The substance of the lungs softened pretty rapidly, and was easily broken up. A portion of it well washed, displayed very prettily the vascular structure of the organs; after about a month or five weeks, little remained but the larger vessels, (some of which retained their minute ramifications,) and the pleura comparatively little altered. Where there were tubercles, the alteration of softening was quickest, and it commenced with the tubercles. It was very difficult to distinguish, during the progress of change, between the bronchia and the blood-vessels, and decide which underwent change most rapidly.

Arteries.—Aorta, (4. 5. 10.) Pulmonary artery, (10.) Cœliac artery, (8.) Femoral artery, (4.)—These vessels underwent change very slowly. The different coats gradually softened, the inner and outer coat first. The inner coat became as it were mucilaginous,

and was washed off or removed by the gentlest friction. The outer coat resisted longer; it finally became disintegrated, and reduced partly to a kind of mucilage-like matter, and partly to its component fibres not distinguishable except on very minute examination of the debris. The middle coat became soft later than the preceding; it wasted irregularly, became full of holes and gradually fell to pieces. During the change, the outer coat assumed a brownish hue; the hue of the inner coat was little altered; the middle coat assumed a bright pink colour. The different arteries exhibited some varieties of change; the pulmonary artery underwent it slowest; the aorta of No. 5, which was diseased, also underwent change very slowly, and in a peculiar manner. The inner coat separated from the middle, and the middle in places from the adjoining fibrous coat, and the middle coat where thickest separated into laminæ. These alterations took place in a space of time varying from six weeks to eight months. It may not be amiss to remark, that by the fibrous coat of the artery, I mean that which is next to the middle, and between it and the loose cellular texture, the envelope of the vessel in which there is generally some fatty matter, and which appears to be even less liable to change than the fibrous, but subject to the same kind of alteration.

Veins.—Vena cava (4. 5.)—It gradually softened; the outer cellular or fibrous coat preceding a little in its change the proper coat or coats of the vein. The softening and wasting process went on irregularly; one portion suffering greatly, and another adjoining very little; and the kind of change was not always the same in the same texture; here a spot became soft, semi-transparent and depressed; there elevated and mucilaginous. In some places it was discoloured brown, in others greenish. Gradually it became extremely thin and worn into holes,—and the change continuing, it was reduced to shreds and particles.

Thoracic duct, (8.)—It slowly underwent change like the vena cava.

Nerves.—I have made but few observations on this subject, and these have been confined to the crural nerve (4.) Its theca gradually softened; in about two months the enveloping and connecting tissue constituting the theca was partially reduced to a brownish mucilaginous state, which was detached on the slightest friction, and then the nerve separated into its component filaments. These filaments slowly lost their strength, became as it were mucilaginous, and fell to pieces.

Pia mater, and arachnoid membrane, (11.)—The latter membrane in this instance was slightly opaque, and consequently the better fitted for observation. In their change from maceration, the two membranes kept equal pace; they slowly lost their strength, in some places more so than others; holes formed in them, which gradually increasing in number and size, in about two months reduced them to shreds. The small vessels ramifying through the pia mater wasted and disappeared much before the larger ones;

these latter remained to the last, but perforated, and undergoing
the same kind of change as the membranes themselves.

Theca spinalis, (8.)—It underwent change slowly. In about
six weeks it presented a very varied appearance; in some places
eroded, as it were, and become very thin; in others, holes had
formed ; close to them, there was a conversion into transparent
spots; and generally there was a fibrous vegetable growth adher-
ing to the surface from which it had sprung. The same kind of
change gradually proceeding, the membrane was reduced to shreds,
and these ultimately to particles of a fawn colour, easily suspended
in water.

Dura mater, (2. 3. 4. 10.)—This membrane, during the first
two or three weeks, did not appear to undergo any change. Gradu-
ally its surface became slimy, either with or without discoloration,
but more frequently the latter. In different specimens, the dis-
coloration was very various, as bright blue, brown, orange, red,
and greenish. A wasting of the substance of the dura mater attend-
ed the formation of the slime, or soft mucilage-like matter; and,
being washed off as it successively formed, the membrane became
very thin and corroded into holes. The change proceeded very
irregularly. The wasting was chiefly confined to particular spots,
as so many centres of action, the adjoining portions remaining little
altered for a long time, and having their strength but little impaired.
In these portions thus exempted in a manner from rapid alteration,
the structure of the tissue was well displayed—the very delicate
connecting fibres yielding to decay first, the coarser fibres became
so much the more apparent. Sometimes the membrane divided
into two layers—occasionally into three or more laminæ; but it
was always easy in this stage to effect its separation into two.
Progressively every part partook of the change, and the membrane
was reduced to shreds and particles. Some of these shreds I have
examined after having been in maceration eleven months. Inter-
nally, when drawn asunder, they displayed the original colour and
lustre and texture of the dura mater, and they themselves appeared
to be in progress to ultimate decomposition. It was curious to
observe the different specimens placed under similar circumstances
undergoing change. Hardly in any two was the change exactly
similar, varying either in the rapidity with which it took place, or
in the discoloration attending it; and this without any apparent
cause, as if owing to difference of composition not appreciable by
the eye. During the early progress of the experiments, I have often
sought to discover, on the inner surface of the dura mater, the
arachnoid membrane; but I was never able to succeed in detecting
it. And I may here remark, that all the other attempts which I
have made to demonstrate it have been failures; and in conse-
quence, I am disposed to believe that it is not reflected over the
dura mater according to the ingenious idea of Bichat.

Pericardium, (3. 9.)—It underwent change like the dura mater;
but rather more slowly in the early part of the process, and more

rapidly in the latter, and without discoloration. Maceration, I may remark, afforded no evidence of its being a compound texture, consisting of an outer fibrous and inner serous membrane, of different characters, as Bichat considered it. That it is a fibrous or fibro-cellular structure, capable of secreting serum, seems certain; but more than this at present appears hypothetical, and deficient in anatomical demonstration.

Cuticle, (4. 10.)—It became detached from the cutis in from ten to thirteen days, and from some parts sooner than from others. It very slowly underwent change. It became discoloured, generally brownish; in one instance partially blue. A slimy matter formed on it, attended with softening; and it gradually fell to powder or small shreds. These changes were best observed in the thick cuticle of the sole of the foot, in which instance they occurred in about twelve months.

Cutis, (4. 7. 9. 10.)—The cutis of different parts underwent change somewhat differently. The difference was most remarkable in the very thin cutis, and in the thick; as in the cutis of eyelids, prepuce, and scrotum, and in that round the anus, the chin, the abdomen, &c. The very thin cutis resisted change for a long time; the subjacent cellular tissue became distended with water, and then it was extremely difficult to say where this tissue terminated, where the cutis commenced; in fact the former passed into the latter, and the latter seemed to be merely the boundary surface of the former, the fibres only more closely compacted than in the loose tissue, and in the instance of the scrotum and the cutis of the penis, impregnated with brown colouring matter. Maceration being continued, gradually the surface became covered with slimy matter, a softening took place, the cutis became thin in spots, and holes formed; and at the same time the subcutaneous tissue lost its strength, became slimy, its fibres separated, and ultimately both the cutis and subjacent tissue were reduced to a sediment of shreds and particles. The thick cutis underwent change more rapidly at first, and slower afterwards. In a short time, as in about a fortnight or three weeks, it became discoloured, of a bright pink or yellow hue, and it exhibited a cribriform structure, very many small holes having formed in it, arranged in a pretty regular manner, penetrating through the cutis. What remained, constituting this cribriform texture, was decidedly fibrous. The fibres were more or less coarse and interwoven, and had the colour, lustre, and general appearance of the fibres of tendons, or of the dura mater when separated by maceration. This rapid change was most distinct in the cutis of the verge of the anus. It may be remarked generally, that the more the integuments abounded in the channels of hair and in fatty follicles, so much the more rapid was this first change or conversion into a cribriform structure. After it had formed, further change was very slow, and in a manner very analogous to that of the thin skin, or of the dura mater and pericardium, and required even longer time for its completion. Between the two extremes of very thick and

very thin cutis, there are parts intermediate in degree of thickness,
and intermediate also in relation to the follicles, &c. which they
contain, as the skin of the inside of fore-arm, and that covering the
shin-bone, that of the ear, not including its lobe, &c.—and these
parts observe also a kind of medium in their rapidity and manner
of change. When examined in a certain stage of the process, they
appear to consist of the same kind of tissue as the subcutaneous,
only a little more condensed; and to the thickest skin the same
remark relative to texture is, I believe, justly applicable.

Alimentary canal, mouth, and fauces, (7. 9. 10.)—Different
parts of the lining membrane of the mouth and fauces underwent
change with very different degrees of rapidity. Where the ducts
of numerous mucous glands pass through the membrane, and
where the membrane is not extremely thin, as in the roof of the
mouth, it soon acquired a cribriform texture like that noticed under
the head of the cutis, and thus perforated, it underwent further
change slowly. Where comparatively few mucous glands are situ-
ated in the subjacent tissue, and where the membrane is compara-
tively thick, as in the side of the mouth, and especially in the neigh-
bourhood of the entrance of the parotid duct, there from beginning
to end it suffered change very slowly. Over the tonsils it expe-
rienced the first or cribriform change very rapidly. The glands
constituting the tonsils were soon converted into a semi-fluid fetid
matter, which was removed by washing, and there remained only
a tissue of strong white fibres. Where the membrane was very
thin, and at the same time penetrated by numerous ducts of mu-
cous glands, as under the tongue, and especially on each side of its
frænum, and in the back part of the fauces, there it underwent
change rapidly, and it was soon reduced to a mucilage-like and
shreddy state. The change which slowly took place in the lining
membrane of these parts, bore a very close resemblance to that expe-
rienced by the cutis, a slimy or mucilage-like matter formed, the part
became irregularly corroded and worn into holes—it lost its strength,
and the whole was gradually reduced to shreds, and these ultimately
to particles, which indeed throughout the change were constantly
forming, and as they formed were removed in changing the water.
And I may remark, that the tissue subjacent to the lining membrane
experienced the same kind of alteration; and the same kind of ob-
servation which induced me to consider the cutis as the external
surface of the tissue connected with it, leads me also to infer that
this membrane is an analogous boundary of the texture beneath it.

Tongue, (7. 8.)—I have not alluded above to the epithelium of
the lining membrane of the mouth; it appears to be very similar to
that covering the upper surface of the tongue. Like that of the
tongue, it varies in thickness and distinctness in different parts. It
is very thick on the roof of the mouth, and on the anterior and
superior part of the tongue; less thick on the sides of the mouth,
or middle part of the superior face of the tongue; hardly distin-
guishable on the surface of the fauces and the back of the tongue,

and not distinguishable on the floor of the mouth under the tongue, and on the inferior surface of this organ. About the third or fourth day, this epithelium began to come away, or was easily detached ; but from no part as a continuous membrane, rather like a curd-like substance, especially from the roof of the mouth. From the tongue, particularly the apex, it might be detached in small portions, having some resemblance to cuticle, and bearing the impression of the surface to which it had adhered. After the separation of its epithelium, the next remarkable change in the tongue from maceration occurred in its muscular substance, which soon became soft and fetid, and in this pultaceous state it was soon washed away in changing the water, when gentle pressure was at the same time made to it. After the removal of this part, there remained its case, as it were, which varied in texture in different parts. On the under surface it was a delicate membrane, apparently composed of cellular tissue, and which soon wasted away. On the upper it had a mixed character, as if composed of tendon-like fibres, and of cellular tissue—the former prevailing most at and towards the apex, and the latter most towards the base, and round the foramen cœcum, and in the anterior part surmounted by papillæ, which, minutely examined, themselves appeared to be bundles of fibres. These papillæ, and this fibrous and cellular tissue, slowly underwent a change very like that described when treating of the dura mater, skin, &c.

Œsophagus, (7. 9.)—In the inferior part of the œsophagus, and especially just at its termination, an outer membrane or epithelium, somewhat similar to that of the mouth, was (as it generally is) distinct; and it separated in the same manner. Higher up it became very indistinct; and I was not able to trace it in a satisfactory manner in the upper portion, and through the pharynx, where, as I have already observed, its appearance is obscure, and its existence often doubtful. During continued maceration, the muscular coat of the œsophagus first disappeared. The inner or mucous coat gradually became soft and corroded. In about two months in winter, its upper part was reduced to shreds, which, stretched under water, had the silky lustre and general appearance of cellular tissue, or of very delicate fibrous structure. The lower part was stronger, and resisted disintegration a little longer. The submucous tissue also resisted it a little longer ; ultimately it was reduced to the same kind of debris, composed of fibres and altered particles discoloured brown.

Stomach, (4.)—During the first eight or nine days there was no distinct softening or change of any part of this viscus. About the twelfth day, the mucous membrane was softening in some places, and was easily abraded from the subjacent cellular layer. This layer, where the mucous coat was removed from it, had the appearance of peritoneal membrane. Its tissue was considerably distended with water. The muscular and peritoneal coats were of a light pink hue. About the eighteenth day, the mucous coat was soft and slimy, and when gently rubbed, was readily detached. About the

thirty-fifth day, the mucous coat was slimy, and of the weakest
consistence; in many places eroded. The adjoining cellular coat,
of a fibrinous reticulated appearance, white, and of a silky lustre,
had lost much of its strength, and was softening. The muscular
coat was not every where distinct; where distinct, there cellular tis-
sue appeared intermixed with it, and it was reddish, and extremely
feeble. The peritoneal coat, externally light brown, internally
white and silky, was also softening. On the forty-fifth day, the
change was in progress in all the tissues; and on the seventy-
second the disintegration was nearly complete; and it was difficult
to recognise any of the coats or layers, excepting the submucous,
which, though almost deprived of coherence, yet was still pretty
distinct; and the larger vessels passing through it were also dis-
tinct, but in the same soft weak state.

Small intestines, (3. 10.)—These parts underwent changes very
similar to those of the stomach, but rather more slowly. On the
thirty-sixth day, in one instance, the surface of the mucous coat of
the jejunum exfoliated in soft brown laminæ, and left exposed a
fresh surface, in which vessels and fibres might be seen, presenting
a very delicate appearance, derived from the submucous tissue, and
terminating seemingly in the mucous or inner coat, in the form of
loops, especially in the valvulæ conniventes, where this configura-
tion was most distinctly displayed, and forcibly gave the idea that
the mucous coat is chiefly formed of vessels, ramifying from and
into the submucous cellular tissue.

Large intestines, (3. 8.)—The changes which they experienced
were also of the same kind, but were even slower in taking place.
The submucous cellular tissue, especially of the rectum, where it
is very thick, retained its form, and considerable strength, after two
months' maceration. The mucous membrane within the verge of
the anus, like the skin at the line of junction, changed more rapidly
than the contiguous part of either texture; and in about twenty-one
days, in the cool season, when the cuticle was in the act of separa-
tion, the softening process had even then commenced. Minute
cavities formed both in the mucous coat and in the cutis, which,
increasing in number and extent, in about a month nearly effected
a separation of the two membranes. This greater tendency to de-
compose in this place of junction, I am disposed to attribute to the
mucous membrane, near its termination, having beneath it an un-
usual number of glandules or follicles; and to the cutaneous mem-
brane in the same situation having a large supply in its substance
of reddish granules, which perhaps are follicles or glandules which
secrete a fluid peculiar to the part. It may be remarked, that I
could not follow the cuticle beyond the inner verge of the anus—
that, when it terminated, it seemed, as it were, to overlap, and to
have no junction with any epithelium similar to that which in the
mouth connects the inner surface of that cavity with the outer sur-
face of the lips.

Gall-bladder and biliary ducts, (2. 4.)—Both the gall-bladder

and the biliary ducts were slow in suffering change from macera-
tion, especially the latter. After twelve days, in the instance of No.
4, they had undergone no apparent alteration. After eighteen days,
when a portion of the liver and pancreas attached to them were in
a state of disintegration, the ducts did not appear to be at all
changed; but the inner coat of the gall-bladder was softening, and,
when gently rubbed under water, yellow particles were detached
from it.   After twenty-three days, the inner coat of the gall-bladder
was slimy, and coming off in flocculi, whilst the ducts were not
materially altered; their peculiar structure remained distinct, the
lancet-shaped valvular processes at and near the mouth of the com-
mon duct, and the transverse and oblique fibres and chords, which
some anatomists have called spiral laminæ, in the cystic duct, were
as distinct as at first, and not perceptibly weakened.   The appear-
ance of the inner surface of these channels bore a strong resem-
blance to the sinuses of the dura mater; and their general proper-
ties, and the manner in which they long resisted decomposition,
and the manner in which they very gradually yielded to it, con-
firmed the analogy. After thirty-five days, neither the hepatic, nor
the cystic, nor the common duct, nor a portion of the pancreatic
duct attached, was altered, except very slightly; the lancet-shaped
processes above alluded to were gradually wearing away.   The
cellular or cellular-fibrous tissue enveloping the gall-bladder and
duct had become very soft; yet, when stretched under water, its
fibres had the peculiar tendinous lustre and appearance. The inner
reticulated surface of the gall-bladder still retained its orange hue;
in many places it was of mucous consistence, or reduced to a slime-
like state.   After fifty-four days, the ducts exactly resembled the
dura mater; the lancet-shaped processes were still distinguishable;
the gall-bladder had lost almost entirely its inner reticulated sur-
face—that commonly called its mucous coat; and there remained
little but a tissue of fibres, which, when stretched under water, had
a silky lustre like tendinous fibres, and retained a greenish yellow
tinge of bile.   After about three months there was little further
alteration, and even after four no very considerable one; the ducts
gradually wasted like the dura mater; a mucus-like substance
formed on them; they became eroded, as it were, and more in some
places than in others, and worn into holes; and the investing tissue
of ducts and gall-bladder became soft and incapable of resistance;
and thus gradually, in from about six to eight months, they expe-
rienced a total disintegration, and were reduced to shreds and par-
ticles of a greenish hue.

  Air passages, (7. 9.)—Different parts of the aspera arteria suffer-
ed change very variously in relation to time.   In the instance of
No. 7, on the twenty-first day there was an appearance of ero-
sion of the epiglottis, and a wasting of the lining membrane of
the trachea, portions of which were in a shreddy state; whilst the
whole of the lining membrane of the larynx appeared to be un-
altered, excepting the sacculi laryngis, the loose tissue in which had

become distended with water, producing an appearance like œdema; and the same was observable round the rima glottidis. In the instance of No. 9, on the twenty-eighth day the erosion of the epiglottis was very similar, but the alteration in the trachea less. On the thirty-second day (in this latter example, to which I shall now confine myself,) the lining membrane of the epiglottis was perforated with small holes, corresponding to glandules underneath, undergoing decomposition, situated in the substance of the cartilage and in the connecting tissue, performing the part of its perichondrium; and similar minute perforations were formed in the lining membrane of the trachea, most remarkably in the back or membranous part of the canal, and I believe from the same cause, namely, softening and disintegration of the mucous glandules, situated in the tissue next to the lining membrane, and communicating by ducts or pores with this membrane. At this time the muscles of the windpipe and the thyroid gland were in a pultaceous state; but the inner membrane of the larynx still remained apparently unaltered. On the fifty-ninth day, the membrane of the epiglottis had almost entirely disappeared; its cartilage was full of perforations, and its margin was much worn and had become soft. The lining membrane of the larynx was here and there slightly eroded, as if ulcerated. When a portion of it was dissected off, and stretched under water, it had the appearance of a fibrous membrane. The chordæ vocales retained their original appearance; and their strength was not apparently decreased. The inner membrane of the trachea had nearly disappeared; the shreds of it which remained had also the character of fibrous membrane when stretched under water, although extremely delicate. The back portion of the lining membrane had most distinctly this character, and it had now acquired a cribriform texture, from the many cavities formed in it. Of the changes which successively took place after the fifty-ninth day, I have not any notes. I may remark generally, from recollection of observations made on other specimens, that the chordæ vocales, the lining membrane of the larynx, the tendon by which the epiglottis is attached to the chordæ vocales at their junction, were the last parts that underwent decomposition; and that they underwent the change in a manner analogous to the dura mater, and the tendons of muscles. The cartilage which yielded first was the epiglottis; next the arytenoid, and the cartilaginous rings of the trachea; and, lastly, the annular and scutiform cartilages; and they underwent the change principally by a process of softening, becoming slowly converted into a mucus-like substance. I may mention that I endeavoured to trace an epithelium from the tongue into the larynx, but in vain; also that I sought for a villous coat in the air-passages, but in vain. The lining membrane appeared to me to be of the nature of a fibrous tissue throughout, and strictly not a distinct membrane—merely the boundary surface of the lining tissue and the component parts. I could detach no membrane from the chordæ vocales; their surface appeared to be the same as their substance—

closely and very firmly connected tendinous fibres. Nor could I
detach a portion in the form or semblance of a membrane from any
part, except corresponding to a comparatively loose fibrous tissue,
in which, or beneath which, are situated the glands, which give the
surface the character of a mucous membrane.

*The passages of the nose and ear, and the cavities connected
with them,* (10.)—I have not paid minute attention to these parts
during a long period of maceration. The few observations which
I have made in one instance, I shall communicate. Where the
lining membrane of the nares joins the cutis, and where the cuti-
cle of skin terminates, there, on maceration during fifteen days, I
have noticed a kind of epithelium, at that time of curd-like consist-
ence, not dissimilar to the covering of the roof of the mouth, admit-
ting of being scraped off, rather than detached as a continuous sur-
face. As it has proceeded inwards, it has rapidly diminished, and
I have not been able to follow it more than half an inch in a satis-
factory manner. The lining membrane of the nasal passages and
cavities has resisted decomposition for a considerable time, and in
some measure according to its degree of thickness. The same re-
mark is applicable to the lining membrane of the passages of the
ear. The membrane of the eustachian tube being comparatively
thick, has borne maceration a very long time; whilst the extremely
delicate membrane of the tympanum has disappeared rapidly. Both
the lining membrane of the nasal passages and of the ear have ex-
hibited generally, during maceration, the character of a fibrous
tissue, with mucous glandules enveloped in them, or subjacent to
them. In the former I have been much at a loss to distinguish be-
tween the periosteum of the cavities and the lining membrane of
the cavities; and in the frontal and maxillary sinuses, and those of
the sphenoidal bone, I am much disposed to believe that they are
one and the same membrane, with mucous follicles intermixed be-
tween their fibres. The membrane lining the tympanum, of such
extreme delicacy and vascular, bears a greater resemblance to the
pia mater than to either a mucous membrane or ordinary perios-
teum—yet it seems to perform the function of both, though I am
not certain that it is provided with any mucous glandules, or moist-
ened with mucus. The membrana tympani appears to be continu-
ous or connected with it; and also continuous or connected with
the cuticular lining of the external passage, which is covered with
cuticle, and which seems to serve for periosteum to the bony ring
of the meatus auditorius.

The eye.—The observations which I have made on this organ
and its appendices during maceration, have also been confined to
one instance (No. 10,) and have been more hasty than I could have
wished. During maceration, when the cuticle has separated from
the eyelids, at the inner boundary of the cilia, I have not been able
to detect any, the slightest epithelium, on the conjunctiva. The
conjunctiva has undergone change like cellular tissue; the sclerotic
coat like dura mater, but more slowly; the cornea has become of a

milky appearance, has gradually softened, and has wasted away more rapidly than the sclerotica. The lens has become soft, and like thick mucus; and the change has gradually proceeded from the circumference to the centre; and the central part, previous to softening, has assumed an opalescent and greenish hue. The membrane of the vitreous humour, before softening, has, like the cornea, acquired a slight degree of opacity, and in consequence has appeared so much the more distinct; it has resisted decomposition many weeks. The lachrymal gland has undergone decomposition very like the parotid; and the lachrymal ducts and sac very like the lining of the nares.

The urinary and seminal organs. The bladder, (4. 7. 6.)—In the instance No. 4, this organ was under observation seventy-two days. Marks of change began to appear on the sixth day, chiefly in the muscular coat. On the twelfth day, this coat was of a light pink hue, whilst the inner coat was white, firm, and apparently unaltered. On the eighteenth day, the inner partially began to soften. On the twenty-third day, all the different coats were soften-ing; the peritoneal was white, the muscular pink, and the inner coat yellowish, with slimy matter of the same hue adhering to it. On the thirty-fifth day, they had all become very soft, and capable of resisting little force; and both the peritoneal surface and the inner surface were slimy. On the forty-fifth day, the muscular coat was reduced to pale soft shreds; portions only of the inner coat were distinct; and these, when examined under water, had the lustre and appearance of the debris of a serous or fine fibrous membrane. The adjoining cellular layer, and the cellular layer connecting the peritoneal coat and the muscular, and the peritoneal coat itself, were much in the same state. On the seventy-second day, very little remained; the small residue not lost in changing the water was in the form of a debris, resembling that of a cellular, serous, or fibrous membrane; and I allude to the three, not know-ing well how to distinguish the debris of the one from the other. The ureters, it may be remarked, resisted disintegration consider-ably longer. During the whole period of change, they very much resembled the biliary ducts, and gradually wasted like fibrous tissue. Whilst the maceration was in progress, I sought in vain for a mem-brane deserving the name of epithelium in the bladder and ureters; and I was as unsuccessful in my search for villi on their lining membrane. I sought also for mucous glands or follicles, but could not detect them in a satisfactory manner in the bladder, excepting a few close to its orifice, in the small triangular space. In some instances they appeared indistinctly in the ureters, in others not. I must confess I have doubts if the lining membrane of the bladder deserves the name of a mucous membrane. In the dead body I have never found mucus adhering to it; and it is easy to conceive that the mucus which is contained in the urine may be derived from the glandules or follicles above alluded to, and from those of the urethra. I express these doubts with hesitation; and chiefly

because it appears wrong to take for granted that which is not proved; and no less so to place much confidence in analogical reasoning, in opposition to, or not supported by, the simple evidence of the senses.

Testes, and their membranes, (7. 12.)—In the instance of No. 7, after eighteen days' maceration, one testis suspended in water, deprived of its tunica albuginea, had become very soft and weak; its structure was well displayed, and its capillary tubes might be drawn out to some length. It appeared to be yielding pretty rapidly to decomposition—but to my surprise it continued pretty nearly in the same state more than a month. On the sixtieth day little of it remained, and that was extremely soft. The tunica albuginea detached from one testis underwent change rather more slowly than the other not detached. Both became corroded, gradually perforated, covered with a mucus-like substance, and a fibrous vegetable growth. They underwent change more rapidly than the tunica vaginalis. On the eighty-ninth day, they were full of holes, and what remained was too weak to support its own weight; it was almost of a mucilaginous consistence, and had acquired a certain degree of transparency. At the same time the tunica vaginalis and the tissue of the scrotum, though somewhat wasted, still retained a certain degree of strength, and when stretched under water, had very much their original appearance—the appearance of a tissue of very delicate fibres, with some coarser ones intermixed, of a silky, or tendinous lustre. During maceration, I attempted to divide the tunica albuginea into two or more layers, but could not succeed. The vas deferens underwent change very like the tunica albuginea; its inner surface, near its termination, which is a reticulated structure, composed of fine tendon-like fibres, resisted disintegration longest. The vesiculæ seminales, too, underwent a very similar change, and their inner fibrous reticulated tissue also resisted change the longest.

The penis, (6. 7.)—On immersion, this organ became distended with water, especially the loose prepuce, and the cells of the corpora cavernosa, and spongiosa. At the temperature of about 60°; after maceration of eight days, the cuticle almost ceased to adhere, and might be detached most easily. I carefully traced it to the mouth of the urethra; about a line inside of the orifice it terminated, much in the same manner as the cuticle terminates in the anus, overlapping the membrane of the urethra. I have endeavoured to find an epithelium as a distinct membrane in the urethra continuous with the cuticle, but in vain; nor have I succeeded in finding any villi in this canal. Few parts of the body resist maceration longer than the penis; and all its different textures are in a very remarkable manner little liable to change, as its skin, its nerves, and vessels, the lining membrane of the urethra, and more especially its ligamentous sheath. Many months are required for the disintegration of any one of these parts; and some of them after twelve months' maceration in water, changed daily, or every second

day, still retained their form, and were only partially corroded or wasted; as the skin of the glans penis, and its internal structure, and as the ligamentous sheath of the corpus cavernosum. The process of change in all these parts appears to be very similar in kind to that of the dura mater, though much slower, excepting the upper part of the urethra, where that membrane is very thin, and where contiguous to it are numerous mucous glandules, which undergo change rapidly, and seem to accelerate its change. The process of change appears to be by the formation of a mucus-like substance, which in the penis I have noticed of very various colours, as pink, dull brown, bright brown, orange, and green. It is preceded by softening of the parts; it takes place chiefly at the surface, or in the portions most exposed to the action of the water; it is not equable; but, as in the dura mater, spreads as it were from certain points or centres, reminding one of ulceration—the part affected seemingly protecting the contiguous part within certain limits from change.

Tendon, (4, &c.)—This part under my observation has undergone change very slowly; and the less mixed it has been with other tissues, so much the less prone has it been to alteration. A portion of the tendo achillis of a limb amputated on the 16th October, 1828, after constant maceration for many months, was little changed; the longitudinal fibres were still strong; and when the adhering brown mucus-like substance, which had formed in small quantity on the surface, was rubbed off, they exhibited their original colour and lustre. The connecting tissue of the fibres had been either destroyed or rendered very soft, and in consequence the longitudinal fibres were easily separated. Tendons, at their origins and insertions, where most mixed with other textures, change more rapidly; but even in these it seems to be rather a disintegration or separation of fibres, than a decomposition of them, and amongst the debris tendinous fibres may always be found, on careful examination, little altered; at least so it has happened in my experiments. The change to which tendon is liable, appears to be the same as that noticed under the head of the penis.

Synovial membrane, (4. 10.)—This membrane has lost its firmness like serous membrane, and its surface has become slimy. With the exception of a few tendon-like fibres, or shreds composed of those fibres and cellular tissue, it has disappeared during a course of maceration of about four months; and the fatty appendices have been apparently converted into adipocire.

Perichondrium and periosteum.—Both these tissues have undergone change in the same manner, and at the same rate in relation to time, as the dura mater, where they have been detached by dissection from the parts which they enveloped. When they have not been detached, their change has been modified by the influence of the contiguous surface—sometimes apparently retarded, and sometimes apparently accelerated. These remarks apply chiefly to the perichondrium of the cartilages of the ribs, and the periosteum of the cylindrical bones. The perichondrium and periosteum of other

parts, according to a variety of circumstances, suffer alteration variously, and especially according to their degree of thickness; thus, when very thin and loose, resembling cellular membrane, the periosteum of the lower jaw disintegrates rather rapidly, like the adjoining cellular membrane, from which it is extremely difficult to distinguish it, and to say where the one begins and the other terminates.

Cartilage, (4. 10.)—From the observations which I have made, cartilage and fibro-cartilage appear on maceration to undergo slowly a very similar change, and to waste away gradually, from being converted into a mucus-like substance. During the change, the colours which I have witnessed in some parts have been various and extremely bright, especially in the cartilage of the symphysis pubis; I have seen it vividly coloured partially brown, orange, blue, green, and yellow.

Bone, (4. 10.)—This structure undergoes change from maceration very slowly, especially the compact bones which contain few vessels and little blood. Changing the water frequently, the animal matter appears to be slowly removed; for there is a constant exudation of a mucus-like substance on the surface, and the brittleness of the bone gradually increases. Various bones, after having been under observation twelve months, had not lost the whole of their animal matter, not even the most delicate, as the fine plates of the ethmoid, and the spongy structure of the turbinated bone.

Intervertebral substance.—I know of no part of the body so little changed by maceration as this. Portions of it from different bodies, which had been in water from eight to twelve months, retained their form, were very little altered in appearance, and had lost little of their substance. The central soft part, almost of gelatinous consistence in the recent subject, was the part chiefly affected; it, in the time mentioned, had principally wasted and disappeared. The outer layers, on the contrary, suffered least; after twelve months they retained, as well as I could judge, their original firmness; and when the minute quantity of mucus-like substance which had formed on their surface was washed off, they exhibited their original colour.

4.— *General remarks and conclusions.*

The changes produced by maceration, or during the process, are referable, I believe, to three distinct causes. 1st, To chemical causes, giving rise to new combinations of the component elements, and to new productions with the aid of the oxygen dissolved in the water. 2dly, To insects—those which constitute the family of Infusoria of Linnæus, (many of which appear to be larvæ) feeding on the animal matter. In many experiments, the larvæ of the common gnat of Malta has been very abundant and active as a destroying agent, performing, in relation to change of form of matter, a part in water similar to that performed by the larvæ of flies on dead animal substances in the air. 3dly, To the operation of vegetation of those

obscure plants, belonging principally to the *confervæ* of the Swedish naturalist, and which in my experiments have invariably appeared, if the process has been at all protracted; and commonly either as fibres shooting from the part in straight lines, sometimes an inch in length, or more obscurely, as a fine velvet-like protuberance, or growth resembling a byssus. The former kind has appeared most remarkably on the neurilema, on the cutis of glans penis, on its ligamentous sheath, on the tunica albuginea, and on the bones, especially the spongy bones. The latter kind has occurred chiefly on the debris of ligamentous and fibrous parts.

The first, or the chemical causes, must be considered as the main ones; and the second and third only as auxiliary, and, in relation to effect, infinitely inferior in activity. The changes chemically produced I have not yet attempted to investigate with any minuteness. I suspect they are in most respects similar to those which occur during putrefaction in the open air, modified in a certain manner by the solvent power of the water, and moreover modified by the removal mechanically by the water of the particles detached, and not dissolved. I have ascertained that the same gases are disengaged from the different textures during putrefaction in the atmosphere, and during maceration or putrefaction in water; and as far as I am able to judge from a slight examination, the insoluble debris, generally resulting in both, does not materially differ. The most striking difference which occurs in considering the two processes is in relation to time, most parts undergoing change more rapidly in the air than in the water, that is, when there is as free an access of air as of water. The difference is probably owing to the water removing a substance which may be called animal leaven, very apt to decompose itself, and to promote the decomposition of animal and vegetable matter with which it may be mixed, and which abounds in and forms a considerable part of all the textures liable to change, and exists more sparingly in those parts which are slow in changing.

XVI.

GENERAL REMARKS ON BICHAT'S THEORY OF MEMBRANES IN CONNECTION WITH THE PRECEDING OBSERVATIONS.

Bichat, in investigating the nature of membranes, paid attention to the effects of maceration, and in his Anatomie Générale he has given the results of his experience. In some instances I perceive the results which I obtained accord with his—in others they vary slightly, and in some they materially disagree. I shall notice only one or two instances of the latter.

He describes tendon as undergoing change pretty rapidly from maceration—at least comparatively. He says it undergoes change

more rapidly than cellular tissue.   And he extends the remark to
analogous textures, as to aponeurosis, and also to the cutis vera,
and to cartilage: he expresses it as his opinion, that if the substance
of these textures were disposed in fine laminæ, like cellular tissue,
and not compacted together, he is persuaded that a maceration of
three or four days would suffice to reduce them to a putrid mass.[1]

Mucous membranes he describes as peculiarly apt to yield to
maceration—more so, indeed, he asserts, than any other animal
substance, with the exception of brain; and he brings forward the
remark, without distinction, without exception, as applicable to the
membranes generally.[2]

It would be of little use to attempt to explain the discrepancy of
these results—I mean of those which I have described as witnessed
by myself, and those referred to of Bichat.   What I have observed,
in multiplied instances, convinces me that pure tendinous fibre, as
regards tendency to change from maceration, is remarkably prone
to it; and the circumstances which I have mentioned in relating
the phenomena, appear to me demonstrative of it—especially one,
viz. when a mass of tendon is disintegrated by maceration, its mi-
nute fibres, after a very long lapse of time, may be picked out very
little altered.

As regards his remark on mucous membranes, relative to the
effects of maceration, it appears to me to be deficient in correctness,
from its extreme generalness.   The observations which I have de-
tailed, show how great is the difference in different mucous mem-
branes, and even in different parts of the same canal in relation to
the effect of this process.

When it is considered how rapidly Bichat pushed his researches—
the extent and the variety of his labours—the short period of his pro-
fessional life—whilst one admires his genius, and admires with asto-
nishment his efforts—one cannot be surprised at his inaccuracies.[3]

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[1] Anatomie Générale, i. 162   [2] Idem, iv. 21.

[3] This extraordinary man, according to M. Husson, was born on the 14th
November, 1771; according to M. Pinel, on the 22d November, 1772.   In
his 22d or 23d year, viz. in 1793, he went to Paris to finish his medical edu-
cation.   A month only after his arrival, he attracted the favourable notice of
Desault, and became his assistant.   This great surgeon died in 1795.   Bichat
then commenced his public life; he collected and edited Desault's works in
an incredibly short time, and that same year gave his first course of anatomy.
In 1800, after eighteen months devoted to research, he published his treatise
on membranes; the same year his treatise on Life and Death; the follow-
ing year his System of General Anatomy; and the commencement of his
System of Descriptive Anatomy.   On the 22d July, 1802, he died after an
illness of a few days, supposed to have been owing in part to inhaling offen-
sive effluvia in carrying on some researches on the putrefaction of animal
matter.   Thus, in the short period of about nine years he ran his astonish-
ing career as an original inquirer.   Pinel, from whose notice of the life of
Bichat these particulars are chiefly taken, states, that during one winter he
opened more than six hundred bodies—and immediately after gave the re-
sults of his observations in a course of lectures on morbid anatomy, in which
he undertook to show that each texture has a peculiar mode of disease form-
ing a part of its peculiar vital character.

Bichat's propensity to generalisation was extreme; it may be considered almost a passion in him; all his writings are characteristic of it. It enabled him to form brilliant hypotheses—most extensive general views—so much the more dangerous on account of their brilliancy—and the ingenuity, and indeed genius displayed in supporting them. No part of his General Anatomy was more elaborated than that embracing the mucous system: it well displays the disposition alluded to. Villi and papillæ ("*villosités* ou *papilles*," he did not distinguish between them,) he considered a part of the character of these membranes—without exception and essential—evidently guided by analogy, by what is witnessed in certain organs, as in the small intestines, as regards villi, and the tongue and glans penis as regards papillæ: whilst, in point of fact, instead of being universal in mucous surfaces, as he generalised, it is more than questionable if they are found any where else, at least in man.

In relation to the general doctrines of Bichat on the subject of membranes, reasoning on the results of the experiments which I have detailed on maceration—the conclusion which appears to me unavoidable, is unfavourable; and it is supported by the results of other experiments—by those on putrefaction, on the action of heat, and of coction, and of such chemical agents the effects of which I have tried, especially of tannin and corrosive sublimate; and also by various physiological and pathological facts and considerations—altogether constituting the kind of evidence which Bichat himself, after he had formed his hypothesis, brought forward to support it. Let us briefly consider the principles of these doctrines:—

1st, Relative to pathology; whence the first idea of a classification of membranes into distinct definite species was derived; it is true, (adopting his distinction of membranes,) that under the influence of disease, the same membrane throughout is uniformly affected; that similar membranes are similarly affected, and different membranes dissimilarly?

2d. Relative to structure:—Is it true, that there are certain simple elementary membranes—as the mucous, the serous, the fibrous, &c. essentially different; analogous in the organic system to the chemical elements, as oxygen, hydrogen, nitrogen, &c. in the inorganic?

3d. Relative to particular membranes:—to take an example; is it true, of the mucous, as Bichat maintains, that they possess an uninterrupted continuity of similar structure throughout?

1st. Of the pathological argument—originally proposed by Dr. Carmichael Smith, adopted by Pinel, and elaborated by Bichat:—in the extensive generalisation to which Bichat carried it, and which was necessary for his grand views, are there not many well established facts opposed to it? If his generalisation were correct, would not the spreading of organic disease and its isolation be very different from what it is found to be? Would the inflammation and ulceration of dysentery be confined to the large intestines? Would there be local inflammations, and ulcerations—not confined to any

particular texture in which they commenced—not spreading along that texture or membrane—but extending in a perfectly opposite manner; in the alimentary canal, for instance, perforating through all the coats into the cavity of the abdomen, giving rise to fatal peritonitis;—in the subcutaneous cellular tissue, to take another instance, not spreading as in diffuse cellular inflammation, through the tissue in which the diseased action commenced, but extending directly outwards, and penetrating through the integuments, as happens in common phlegmon? Would, in one disease, various and dissimilar organs be affected—as happens in so striking a manner in tubercular phthisis—to the extent, to baffle calculation as to the limit of complication; and as happens, in the majority of instances of the exanthemata, when they prove fatal, especially in small-pox, measles, scarlatina, and erysipelas, the fatal termination of which is commonly owing to the supervention of disease in some deeply seated organ, frequently never suspected during life—not having been indicated by any well marked symptoms?

The evidence derived from pathology dispassionately considered, appears to me on the whole far stronger against, than for the peculiar views of Bichat. Guided by these views, one cannot proceed a step in the careful examination of disease in connection with structure without encountering difficulties. I shall notice in the briefest manner only a very few—and for the sake of briefness in the form of questions. Why is the aorta so frequently diseased and the pulmonary artery so rarely, their structure being similar? Why is the upper part of the duodenum so rarely the seat of ulceration, and the lower part of the ileum so frequently, and still more frequently the vermiform process of the cœcum, all three being glandular, but the duodenum in the most marked degree? Why in tubercular phthisis are ulcers so frequently found in the larynx, and so unfrequently in the trachea? Why is the urethra so little liable to ulcerate? Why are the ureters almost exempt from ulceration?

2dly. On the structure of membranes, viewed as elements—simple and uniform—well marked by their appearances and properties —perfectly distinct from each other, and incapable of being confounded;—in colour, thickness, hardness, density, resistance, &c. nowise similar;—subjected to desiccation, putrefaction, maceration, coction, &c. offering different results—in brief, according to Bichat's idea, each as well characterised as is a simple substance, or chemical element: on this subject it appears to me there is much want of satisfactory proof, and the weight of evidence is against the conclusion. How little analogous for instance is the lining membrane of the ureters and of the small intestines; of the gall-bladder, and of the urinary bladder; of the frontal sinuses, and the other adjoining sinuses, and of the stomach and duodenum; of the stomach and duodenum, and of the larynx and trachea. Some of these in structure, appearance, firmness, the manner in which they undergo change and are affected by reagents have a greater resemblance to fibrous membranes than to mucous—especially the lining mem-

brane of the ureters, of the larynx, of the sinuses. In the frontal
sinuses, I have not been able to detach a mucous membrane from
the periosteum; the periosteum and mucous membrane appear to be
one—the mucus I believe being derived from follicles contained in
the tissue of the periosteum.   In the ureters, it appears to me very
doubtful, that the lining membrane is a mucous membrane.   In
character it appears to be a delicate fibrous tissue: I have never de-
tected mucous follicles either in its substance, or beneath it, or mu-
cus in its canal; and I have often carefully sought for both.[1]   It
would be easy to multiply instances, of the deficiency of well
marked character—subversive, I apprehend, of the generalisation
in question.   In the scrotum, the cutis vera seems to be merely the
boundary of the cellular tissue of the scrotum, externally; the one
insensibly passes into the other.   A similar remark applies to the
periosteum of the maxillæ, especially of the inner surface; the mem-
brane there covering the bone, seems cellular membrane only a lit-
tle condensed, and differs very little in character, in the instances
in which I have examined it, from the contiguous cellular mem-
brane.

3dly. Take the instance of a particular membrane, and examine
it according to the views of Bichat—as to the continuity and iden-
tity of structure—and see what will be the result in point of evi-
dence; and let it be a mucous membrane as before proposed.   If
the alimentary canal be selected, how very different will its differ-
ent parts be found; the œsophagus with its epithelium, without pa-
pillæ, without villi; the duodenum with a villous surface, over a
glandular structure; the jejunum with its villous valvulæ conni-
ventes, the colon without villi or papillæ, without valvulæ conni-
ventes, possessed of solitary mucous follicles.   If the air passages
be chosen, the results of the examination of the different parts will
hardly be more dissimilar; as of the under surface of the epiglottis
compared with the cordæ vocales; or the surface of these compared
with that of the trachea beneath; or that of the trachea compared
with the surface of the bronchial branches.   In the different parts
of the mouth, the differences of structure are even more strongly
marked, as the upper and under surface of tongue—the one covered
with a thick epithelium—the other I believe altogether destitute of
epithelium. The surface of the amygdalæ presenting fibrous cryptæ,
without epithelium; the surface of the palate, a dense fibrous struc-
ture, not I believe distinguishable from the periosteum, abounding
in glandules covered with a thick epithelium; that of the uvula, de-
licate and yielding, little more than loose cellular tissue, connected
with muscles; that of the cheeks, firm and dense, and like the
parotid duct very analogous to a fine fibrous structure.   An exa-
mination of the eye, would call forth similar remarks: the transpa-

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[1] In accordance with this it is liable to adhesive inflammation:—In two
instances I have known the ureter closed by the adhesion of its lining mem-
brane.

rent cornea is continuous with the opaque sclerotica; the conjunc-
tival lining with the lachrymal ducts; how strongly are they con-
trasted! If from man the examination were extended to other
animals, the differences presenting in continuous surfaces would be
found to be even more striking—as in the several stomachs of the
ruminating animals—the gizzard of the gallinaceous tribe—the
œsophagus of many of the tribe of cartilaginous fishes. How totally
inimical to the doctrine of Bichat, is the second instance alluded to,
in which the continuity of mucous membrane is entirely interrupted,
and its place supplied by a horny lining.

M. Magendie, in his excellent notes to Bichat's Treatise on Mem-
branes, has made remarks to the same effect. He remarks admir-
ably on this very subject, "that generalisation carried too far creates
difficulties. That it is clearly useful to study mucous surfaces as
one and the same membrane extended over different organs; but,
that when the examination of the structure of the membrane is
minutely investigated in detail, marked differences occur. In truth,
(he adds) mucous membranes differ essentially, according to the
organs, and even the parts of organs to which they belong. The
mucous lining of the œsophagus does in nothing resemble, it may
be said, that of the stomach or nose. The lining membrane of the
small intestines is totally different from that of the large, or of the
bladder of urine; and it is precisely the points constituting the dif-
ference which ought to be remarked, much more deservedly so than
the points of resemblance, which are very slight. In animals (he
proceeds) the characters are even more strongly opposed; in the
stomach of the horse, the splenic portion is entirely different from
the membranous pyloric portion. Each of the four stomachs of the
ruminants offers a very different disposition of structure;—to be-
come acquainted with the function of each, it is necessary to study
the peculiar structure of each. In general, it is of much more
importance to be acquainted with the differences than with the
analogies of things."[1]

In this note, M. Magendie, it appears to me, has designated well
the defective part of the doctrines of Bichat—generalisation—which
was his strength, indulged in to excess, constituting his weakness.
His doctrine of membranes may be considered as an hypothesis;
such it was originally, and such I apprehend it is still. A few pa-
thological observations originated the notion of distinct elementary
membranes; analogy led to the idea that they would be found dis-
tinct in structure and properties; experiments and observations
were made in expectation of confirming the analogy, and it accord-
ingly was in a certain manner confirmed; as M. Magendie remarks,
the points of resemblance, which were slight, having been seized
—those of difference, often great and always more important,
neglected.

[1] Traité de Membranes, Nouvelle edit. revue par M. Magendie. Paris,
1827. Note p. 33.

Bichat's theory of membranes often reminds me of the antiphlo-
gistic theory of Lavoisier; as simple, as logical, admitting the pre-
mises; and, viewed as an effort of mind, as great an effort of genius;
and like it a too hasty generalisation, that is, instead of being an
induction of facts, it is in many respects a presumption—the anti-
cipatio mentis, built up in great part *a priori;* and having perhaps
the same tendency to shackle the mind and check free inquiry.
Bacon profoundly remarks, " Credunt homines rationem suam ver-
bis imperare; sed fit etiam ut verba vim suam super intellectum re-
torqueant et reflectant quod philosophiam et scientias reddidit so-
phisticas et inactivas."

The investigation of the subject of membranes and tissues is un-
doubtedly one of great importance, and deserving of most careful
attention.   Something has been done in the inquiry, but I appre-
hend, little in comparison with what remains to be done, to bring
it to a satisfactory conclusion.   In proof, it may be mentioned, that
no classification of membranes yet proposed by the many followers
of Bichat, has been generally received, or has obtained any but par-
tial assent.   Inquirers are divided even in opinion respecting the
nature of particular structures; as for instance, that of the middle
coat of arteries—some considering it analogous in nature to the
substance of the muscles over which the will exercises no control,
and in which the peculiar arrangement or appearance of fibres observ-
able in the voluntary muscles under the microscope is not distin-
guishable: others, holding it to be totally different from muscular
substance, neither irritable nor contractile, but elastic, and most
analogous to elastic ligament, as the ligamentum nuchæ.   This
difference of opinion has arisen out of various research, not carried
sufficiently far; and is a good example of the state of our know-
ledge of the subject of membranes generally, in which a no small
number of points are open to question, and comparatively few are
fixed.   Another example presents itself in mucous membranes,
viewed in the different relation.   It is yet undecided, whether or no
the secretion from which they derive their principal characteristic
and their name, is solely the production of glandules or follicles
disseminated through their texture, or that of the submucous cellu-
lar tissue; or in part that of the membranes themselves independent
of such follicles or glandules.

In a former section, namely, on the effects of boiling water, and
of long-continued boiling on animal textures, I have expressed the
opinion, that the results detailed relative to the white membranes
and fibrous parts, seemed to me rather in favour of Haller's early
view of them, than of the later and more elaborate one of Bichat—
not that they are all distinct tissues, each sui generis—but that they
are analogous in composition, and differ rather in degree than in
kind, and that their basis is cellular tissue.   When their properties
are considered, traced, as it were in transition; and when it is con-
sidered how easily animal and vegetable substances are modified,
there is little difficulty in arriving at this conclusion.   Starch, it

would appear, by some change of molecular arrangement, its che-
mical composition remaining unaltered, becomes sugar; the change,
as it is well known, is produced artificially by boiling it in water
acidulated with sulphuric acid. Tendon by simple coction is con-
verted into gelatine; and all the other white fibrous and mem-
branous parts by boiling, are more or less similarly changed in their
properties, without being altered, as far as has yet been ascertained,
in their chemical composition. And, the conclusion, I apprehend,
is strengthened, by reference to pathology, physiology, and compa-
rative anatomy, and especially the last.

The test of truth is practical usefulness. No systematic arrange-
ments of membranes or tissues yet promulgated, based on the views
of Bichat, can claim this proof; or be considered otherwise than
hypothetical. The desideratum now appears to be a careful and
unbiassed examination of every different animal structure apart as
much as possible from hypothetical views, and the application of the
inductive method to the results.

XVII.

OBSERVATIONS ON THE STRUCTURE OF THE DUCTUS COMMUNIS CHOLEDOCHUS; AND ON THE FLOW OF THE BILE.

Many years ago, when engaged in minutely examining the com-
mon biliary duct and the pancreatic duct in the human subject, I
observed a peculiarity of structure belonging to the former, and
partly shared by the latter—which, I believe, had not before been
noticed—at least, I have not been able to find it described in any of
the works of anatomy, which I have consulted expressly with this
object in view.

The peculiarity of structure alluded to, may be brought into
view, by slitting open the tubes just mentioned and inspecting them
under water, after the removal of adhering mucus by washing.
Thus prepared, the lower parts of the ductus communis choledochus
and of the pancreatic duct, will be seen provided with a kind of
valvular apparatus, composed of delicate angular processes or pro-
jections of their inner coat, pointing downwards.

Since 1827, when my attention was first directed to this struc-
ture, I have examined the ducts in question in a large number of
instances: I have invariably found it to exist; but sometimes more,
sometimes less developed. The processes, or valvular projections,
commonly first make their appearance (tracing the passage down-
wards from the liver) a little above the place of junction of the pan-

creatic duct, and are continued to the termination of the common duct in the intestine. In the pancreatic duct, they are more limited; they are confined to its mouth, and occasionally to a small portion of the tube adjoining, not more than two lines from its mouth.

Their valvular nature is easily shown by the introduction of a probe; directed downwards, towards the intestine, it passes smoothly without obstruction; but in the contrary direction, towards the liver, it is impeded and stopped, arrested by the minute sacculi, which exist at the base of the processes.

The use of these processes is implied in their valvular nature and situation: whilst they do not stop the descent of the secreted fluids, which it is the office of the ducts to discharge, they are well adapted to prevent the pancreatic juice from ascending either into the gall-bladder or the liver, and the bile from flowing or penetrating into the pancreas.

I have been desirous of learning if a similar structure exists in other animals; but I regret to say, that in no work on comparative anatomy which I have yet consulted, whether systematic or special, have I found the termination of the biliary duct minutely described, so as to be able to compare it with the human. The few observations, which I have had an opportunity of making, seem to show that this duct at and towards its termination in the intestine in animals of different families, is somewhat various. As the varieties tend to confirm the use of the valvular structure in man, I shall briefly notice them.

In the cat, the biliary and pancreatic tubes are very similar to those of man in the manner in which they terminate; and they are similarly provided with valvular processes, and proportionally, even more strongly developed.

In the ox and sheep, the pancreatic duct terminates apart from the common gall-duct. In the gall-duct of the former, the processes are shorter comparatively than those of man, thicker and rounded, and are confined to the margin of its mouth. In that of the sheep, there is hardly a trace of them, even at the mouth of the duct [1]

In the horse and elephant, both of which, as it is well known, are destitute of a gall-bladder, the terminal structure of the ducts under consideration is very similar. It is distinctly valvular, and in a manner bringing to recollection the valvulæ conniventes of the spiral intestine of the cartilaginous fishes.[2]

[1] The pancreatic duct in man, at least of Europeans, rarely terminates apart in the intestine; in one instance in which I found it so terminating, the valvular structure of the ductus communis choledochus was unusually indistinct.

[2] In the horse the hepatic and pancreatic duct terminate in a sinus or pouch-like cavity, which may be considered as belonging to the intestine— not unlike in its conformation the cavity of the caput gallinaginis of the human urethra. In the elephant their termination is very similar: the mouth of the sinus is circular and sufficiently large to admit the end of the little finger; the cavity is cup-like within.

In the turkey, two or more hepatic ducts enter the intestine direct and apart; terminating in the form almost of capillary tubes, prominent and flexible with extremely small orifices, in appearance not unlike the membranous terminal portion of the urethra of the ram projecting from the corpus spongiosum, they are without a valvular structure.

In the ray (raia batis) the structure of the biliary duct, where it passes obliquely through the coats of the intestine, near the pyloric end of the stomach, is distinctly valvular, exhibiting transverse folds or plates, offering resistance backwards only contrary to the course of the bile.

Besides the use assigned to the valvular processes in man, to prevent the reflux of the fluids which the biliary and pancreatic ducts are designed to discharge, they may be auxiliary also, towards two other objects, viz. in offering an impediment to the contents of the duodenum entering the common duct—and in retarding the flow of the bile from the liver into the intestine, and so promoting at certain times its passing into the gall-bladder.

In the examination of bodies, I am not aware that chyme has ever been found in the ductus communis choledochus; I have never seen it there. And from what I have observed, I am disposed to believe, that the bile, after death, rarely if ever descends into the intestine, but is retained in the gall-bladder and ducts above the entrance of the common duct between the coats of the intestine. This I infer from finding the penetrating portion of duct, not stained with bile, like the portion above, and therefore it may be presumed not containing bile.[1] I mention these circumstances in proof that the structure of the terminal portion of the duct is such as to act in the manner above mentioned. Besides the valvular processes described, several circumstances without doubt are involved in the operation—as the common duct, previous to entering the intestine being enveloped in, or attached to a portion of the pancreas; its oblique passage through the coats of the intestine; the diminution of its diameter there; and its clitoris-like papilla, protected by a fold of the villous coat of the duodenum, somewhat resembling a prepuce, as has been pointed out and described by Morgagni:[2] and in relation to the gall-bladder, in addition to some of these circumstances, its position, partly protected from the pressure of the abdominal muscles by the margin of the liver; and, perhaps, the relative position of the common duct, having close to it on one side the hepatic artery, and beneath it the vena portæ: and perhaps also, the somewhat spiral and lamellar structure of the cystic duct.[3]

[1] This portion of the common duct, may be considered in function analogous to the rectum, commonly empty and collapsed, and distended only just before, and in the act of evacuation.

[2] Epist. Anat. p. 33.

[3] The biliary ducts exhibit in different subjects, several varieties—indeed, they are rarely perfectly uniform, either in dimensions, or the manner in which they are connected, and especially the common duct with the pan-

I shall conclude with a notice of some experiments which I made in 1829 on the dead body, connected with the points under consideration.

Experiment 1st.

Applied a ligature to the pylorus, and made a small opening in the hepatic duct, just above the junction of the cystic, and successively distended by means of a powerful syringe the duodenum with water and with air: neither passed into the duct.

Experiment 2d.

After the duodenum had been well washed out, it was filled to distention with clear water. Pressure was made on the gall-bladder and ducts; the water in the duodenum was found strongly coloured with bile; and bile was found to pass when similar pressure was used, the duodenum being distended with air.

Experiment 3d.

Laid open the duodenum, so as to expose the papilla of the ductus communis coledochus, and having tied the hepatic duct, made pressure on the gall-bladder:—the common duct was presently distended with bile, but no bile passed into the intestine; a considerable degree of pressure was required to make it flow into the intestine. And the result was the same, and even more strongly marked, when the hepatic duct was not tied.

creas. In illustration of some of the circumstances mentioned in the text, I shall give the results of a careful admeasurement of one ductus communis choledochus—from a man, aged 30, who died of pulmonary consumption. Slit open, the greatest width of the tube, at the junction of the hepatic and cystic ducts, was $\frac{76}{100}$ of an inch, its least near its termination $\frac{30}{100}$; its length, measuring from the junction of the tubes, was just three inches; rather more than one-third of its length, viz. $1\frac{6}{100}$ inch, was hid, enveloped in the pancreas and penetrating the coats of the intestine; where it first joined the pancreas, its width was $\frac{42}{100}$; the length of the portion penetrating obliquely the intestine was $\frac{40}{100}$; and the longest valvular filament or process was $\frac{18}{100}$. In this duct, the valvular apparatus was strongly developed, and when the processes were distended (as they were by immersion in dilute sulphurous acid)—their points crossed obliquely, completely obstructing the passage. Glisson in his Anatomia Hepatis, mentions, or rather infers the existence of, circular fibres, in the oblique part of the duct, to which he attributes in part the prevention of regurgitation, "regressu omnis in ductum communem præpeditur à fibris annularibus, quæ non modo orificium ipsum, sed et totum obliquum tractum obsident."[i] In the duct, just noticed, there was a distinct appearance of transverse fibres, but to a very limited extent, close to the mouth of the pancreatic duct; they had a silky lustre, not unlike the fibre of tendon or of the dura mater, and their use, it may be conjectured, is to prevent undue extension of the part.

[i] Anat. Hepat. Cap. xvi.

Experiment 4*th.*

After having laid open the duodenum, and emptied the gall-blad-der, by a small opening in its fundus, which was closed by ligature, injected water *gently* through the hepatic duct, it rose more freely in the gall-bladder than it passed into the duodenum.

Experiment 5*th.*

Placed the body in such a position that the gall-bladder was raised above the level of the mouth of the common duct, and in-jected water as before.   Now it flowed into the duodenum, but did not enter the gall-bladder.

Experiment 6*th.*

The body lying on its back, injected water forcibly, and with va-rious degrees of force ; it flowed freely into the intestine, and rose in the gall-bladder so as to fill it nearly one third full,—and to the same extent, whatever was the degree of force employed ; and which appeared to be (as well as could be judged by the eye) the level of the mouth of the duct, supposing a horizontal line to be drawn from it to the gall-bladder.

Whether there is any vital, peculiar force regulating the expan-sion and contraction of the mouth of the common duct, and conse-quently the flow of the bile, it is difficult to determine.   That me-chanical causes must operate powerfully can hardly be doubted :— the results which I have described seem to prove it; and such causes in conjunction with the variable secretion of the biliary fluid, ac-cording to the degree of activity of the secerning organ, will proba-bly serve to account for most of the phenomena belonging to the course of the bile both in health and disease.

XVIII.

OBSERVATIONS ON PUS.

The observations which I propose to offer on this subject are of a miscellaneous kind, with the hope of contributing something to ad-vance our knowledge of this very important fluid.

1.—I have made several experiments to endeavour to determine if the pus of an abscess contain air. In each instance the pus, as it flowed from the abscess, carefully opened at the moment, was received into a vial full of distilled water that had been subjected to the air-

11—d   15 *dav*

pump until entirely deprived of air; and as soon as it had acquired
the temperature of the atmosphere, it was submitted to the action of
the pump, the same instrument as before noticed, and in good order.

The first trial was on thick pus from an abscess in the neck of a
young man recovering from an attack of erysipelas of the face. The
quantity received into the water was small; it remained at the
bottom and was perfectly tranquil; not a particle of air was disen-
gaged from it, when the exhaustion was as complete as possible.

The second trial was made on pus from an abscess in the arm of
the same patient, opened the day after the preceding; and in this
instance also the result was entirely negative.

A third trial was made on pus from the same person, obtained
from an abscess which had formed in the axilla, opened three weeks
after the last, when his convalescence was well advanced; and again
the result was negative, not a particle of air was disengaged from it,
in the most perfect vacuum that could be formed.

The fourth trial was on pus from an abscess in the arm of an old
soldier, after a very severe attack of erysipelas, (diffuse cellular in-
flammation,) which for many days had endangered life. The mat-
ter in this instance was rather thick, and it did not subside readily in
the water. Submitted to the air-pump, three or four bubbles of air
rose from it. This occurred before the vacuum was nearly com-
plete; seeming to indicate, that the air did not belong to the pus, but
was accidentally entangled, in passing from the abscess, into the
vial; and in confirmation it may be remarked, there was no general
effervescence, or separation of air from the pus, even of the slightest
kind, when the exhaustion was carried as far as possible.

These trials then tend to prove the absence of air in pus from an
abscess. If, however, air in any way have access to the purulent
fluid, and it be subjected to the air-pump, the result, as might be
expected, will be different. In one instance, I had proof of this in
an experiment with the air-pump on some pus from the pleura, in a
case of empyema complicated with pneumothorax, of many months'
duration, in which an opening had spontaneously formed in an in-
tercostal space, discharging daily a small quantity of purulent fluid
of good consistence and colour; although entirely free from fetor,
this pus under the exhausted receiver gave off a good deal of air.[1]

[1] Some pages back I have given the results of experiments apparently
showing that air is not contained in the animal solids and fluids in their
normal state. I perceive, that Professor Burdach has arrived at a different
conclusion, and that he refers to Dr. Dalton in support of it. In the 8th vol-
ume of his Physiology considered as a Science of Observation (cap. ii. 4.)
he remarks: "There are many inorganic bodies, especially water and the
earths, which absorb air, and sometimes even to an extent exceeding their
own volume, and consequently condense and render it latent. The organic
substance also exhibits an attraction of this kind for air in a very high degree.
Dalton (Bibliotheque Univ. de Gen. t. liv. p. 130) has proved in the following
manner, the existence of a considerable quantity of air in combination in the
human body.

"The lungs and stomach are the only parts of the body which contain free

2.—It is commonly stated that the specific gravity of pus is 1030.[1] According to my experiments this quality is far from fixed; I have found it considerably variable. I shall state some of the results which I have obtained.

i.—From the pleura, in the case already referred to, of empyema, complicated with pneumothorax, it was of specific gravity 1028; it was pus of good quality and ordinary consistence, " Album, leve et æquale."

ii.—From an abscess in the thigh, not quite equable, its specific gravity was 1031.

iii.—From the axilla, in convalescence from erysipelas, pretty equable, of moderate consistence; it was of specific gravity 1029.

air; the former with a capacity of 100 cubic inches, the latter of 50; but the volume of the whole body is about 4500 cubic inches, or setting aside the lungs and stomach, 4350 cubic inches for the whole of the remaining solids and fluids. Now, as the mean specific gravity of these is 1050, their weight for their volume ought to be equal to 4567 cubic inches of water, that is, 164 lbs.; but the real weight of a man alive of the bulk in question is only 146 lbs. that is equal to 4044 cubic inches of water; consequently it is necessary that the solids and liquids during life should be penetrated with air; and that thus they should attain a specific gravity inferior to that which belongs to them when examined apart, detached, after death, when a portion of their air is disengaged."—" Dalton (he continues) states further in support of this argument, that when the hand is applied to the orifice of a receiver on the plate of an air-pump, on exhaustion, it is felt to be drawn in and to swell, because the air contained in the part tends to escape."

Neither the argument used by this distinguished man, nor the example, I apprehend, can with propriety be opposed to the results of actual experiments. But waiving this, are the data on which he reasons, established in fact ? Is the mean specific gravity of the solids and fluids so high as 1050? And is the specific gravity of a dead part detached, independent of difference of temperature, higher than before it was detached and during life? I believe a sufficient number of accurate experiments have not yet been made to determine the first question. The second, I believe, must be answered in the negative; from all I have witnessed in trials with the air-pump, I am convinced, that instead of air being expelled after the excision of a part and after death, and the specific gravity of the part increased, the contrary happens,—air enters into it, especially its divided vessels, and its specific gravity is diminished. Even admitting the data, the reasoning seems defective, inasmuch as though the specific gravity of the whole body must be diminished by the penetration of air, the weight of the whole body must be increased by it. Relative to the drawing in of the hand and its apparent swelling, in the instance adduced, the effect admits of easy explanation, by reference to the operation of atmospheric pressure disturbed: on the side from which the air is exhausted, resistance is withdrawn, the hand is pressed in from the force acting from without, and if a blood-vessel be opened, [as is witnessed in the operation of cupping, the vessels being subjected to pressure on one side and not on the other,] the blood will flow freely in vacuum. I have already given the result of subjecting portions of vessels containing blood, to exhaustion, not partial but on every side, indicating no expansion, no swelling.

[1] Dr. Pearson found, in two instances, a certain relation between the specific gravity of the pus, and of the serum of the blood of two individuals ; in one, that the pus was of specific gravity 103., the serum was of specific gravity 1029 ; and in the other in which the former was 1033, the latter was 1031. —Phil. Trans. for 1810, p. 295.

iv.—From the arm, also in convalescence from erysipelas of a dangerous character, (before alluded to, as well as the last,) its specific gravity was 1036.

v.—From an abscess in the back of a young man, its specific gravity was 1040; it had a slight reddish tinge, and under the microscope was found to contain a very few blood corpuscles.

vi.—From a large cavity in the lung in a fatal case of pulmonary consumption, its specific gravity was 1042; it was rather thicker than the healthy pus of an abscess.

vii.—From a vomica in the lung, in another fatal case of pulmonary consumption, it was of specific gravity 1021.

This variableness of specific gravity is probably owing to two circumstances, chiefly,—to variable proportions of the ingredients of the specimens tried, and the variable density of the liquid part. If, cæteris paribus, the liquid part of the pus predominates, it will probably be of comparatively low specific gravity; if the concrete part is in excess, the pus-globules, it will probably be of high specific gravity.[1]

The pus-globules themselves, I believe to be not less heavy than the blood corpuscles; I have come to this conclusion by mixing small portions of pus with solutions of sulphate of soda, of different degrees of strength; with one of specific gravity 1132, the globules were for a time suspended, slowly subsiding rather than rising towards the surface; and, in this instance, when examined by the microscope, the form of the globules did not appear to have undergone any distinct change.[2]

What the specific gravity of the liquid portion is, I have never ascertained in any example; it is, indeed, a point difficult to determine, unless a large quantity of purulent fluid can be employed for procuring it; for the concrete part, the globules, and the liquid part, cannot be separated by filtration, owing to the viscid quality of the latter; and owing to the same quality, the globules on rest subside very slowly, and rarely leave the supernatant fluid perfectly transparent; the supernatant fluid, under the microscope, is commonly found to contain globules, the majority of them smaller than the ordinary pus-globules, and of a form less uniformly regular.

3.—As might be expected, pus freezes less readily than water. In the winter of 1811, I exposed some pus from an abscess, to the open air over night, when the temperature was several degrees below 32°. On the following morning, it had an appearance not unlike that of frozen milk, semi-hard, easily broken, composed of

[1] Dr. Pearson, in his paper entitled "Observations and Experiments on Pus," in the Philosophical Transactions for 1810, notices four kinds of pus.—1. The cream-like, and equally consistent; 2, the curdy and unequal in consistence; 3, the serous and thin kind; 4, the thick, viscid, or slimy. He states that he found the specific gravity of each kind very similar.

[2] Concentrated solution of sulphate of soda, imparts viscidity to pus,—but more slowly and in a less degree than muriate of ammonia, and appears to have less effect on the general appearance of the pus-globule.

large thin plates with fluid intermixed, arranged vertically. On thawing, the pus did not recover its original properties. Previously it was not viscid, excepting in a very slight degree ; now it had become so distinctly, and was slightly ropy. It was also partially coagulated ; and it mixed less readily with water.

4.—Certain fluids, mixed with pus, have the effect of rendering it viscid, like mucus. Solution of potash and soda, and ammonia, act thus in a remarkable manner, and a solution of muriate of ammonia and other neutral salts act similarly, but in a less degree. These are facts which have been long known. Two views have been taken of the effect.

It was supposed by Mr. Hunter, who first noticed specially the phenomenon, that it was an instance of coagulation,[1] an opinion which has been adopted by some recent writers ; thus one, deservedly eminent, (M. Andral) in an article on pus in the Dictionnaire de Médicine, noticing different varieties of purulent fluid, remarks,— " Tous en définitive, paraissent être composés de globules semblables, nageant dans un fluide, lequel est susceptible de se coaguler, lorsqu' on le mêle à une solution de muriate d'ammonique." Dr. Pearson, on the other hand, has denied that it is a coagulation, and supposes it to be an inspissation, "seemingly occasioned," as he says, " by the solution of muriate of ammonia attracting water from the pus."[2]

From the observations which I have made, both these modes of explaining the effect appear to be inadmissable. When I had mixed a little of the fluid part of the pus, decanted tolerably clear, with a solution of muriate of ammonia, no change was produced, not the slightest degree of viscidity was imparted ; but when some of the opaque part containing globules was added, it shortly became viscid. And in confirmation I may remark, that when viscidity is imparted to pus either by the saline solution just mentioned, or of common salt, or of nitre, the quality is not destroyed by dilution ; the greater part of the saline matter may be removed by repeated

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[1] Mr. Hunter describes pus as a liquid, " composed of very small round bodies, very much like those small globules which, swimming in a fluid, make cream." He adds, "these globules swim in a fluid, which we should at first suppose to be the serum of the blood, for it coagulates with heat like serum, and most probably is mixed with a small quantity of coagulating lymph ; for pus in part coagulates, after having been discharged from the secreting vessels, as mucus is observed to do. But although it is thus far similar to serum, yet it has properties that serum has not. Observing there was a similarity between pus and milk, I tried if the fluid part of pus could be coagulated with the juice of the stomach of other animals, but found it could not. I then tried it with several mixtures, principally with the neutral salts, and found that a solution of sal ammoniac coagulated this fluid : not finding that a solution of this salt coagulated any other of our natural juices, I concluded that globules swimming in a fluid that was coagulable by this salt, was to be considered as pus, and would be always formed in sores that had no peculiar backwardness to heal."—(Treatise on the Blood, &c., in vol. iii. of Works, p. 449.)

[2] Philosophical Transactions, for 1810.

additions of water, and yet the pus will remain viscid; and further, that if pus be mixed with a large quantity of water in a tall jar, and the water be decanted after the pus globules have subsided, and this repeatedly till the liquid part of the purulent fluid is entirely removed, the globules collected on a filter become viscid on admixture with a saline solution, almost as readily as if they had not been so treated, and not in an inferior degree.

5. Dr. Pearson makes a statement apparently contrary to what I have advanced; he says, if the purulent fluid rendered viscid by a strong solution of muriate of ammonia be agitated in cold water, " the matters are diffused, and on standing, the pus is precipitated in its original state;" and also, that viscidity is not imparted by a dilute solution of the salt. In answer to this, I shall notice the appearances I have observed in making an experiment, to test the value of his remark. A strong solution of muriate of ammonia was diluted with rather more than an equal volume of water, into which, so diluted, a little pus of an abscess was poured, and they were mixed by agitation. The mixture was of a light grayish hue, translucent and equable; the globules showed no disposition to subside, marking a slight degree of viscidity, and which, on trying it with a probe, it was found to possess. It was now thrown into a large quantity of water, by which its viscid property became very conspicuous. It immediately sunk, and did not mix with the water, keeping together in a mass like the glairy white of an egg, similarly treated, or like mucus, which, under water, it greatly resembled; and the resemblance was more striking from filaments being raised from the general mass, by minute air bubbles disengaged and suspended in the viscid matter.

As the concentrated solution of muriate of ammonia and the other neutral salts, in my experiments, have not rendered the fluid of pus viscid; as they do not render serum viscid, and there appears to be no necessary relation between concentration and viscidity, the conclusion I have come to, and which has been confirmed by all I have witnessed, is, that the effect of viscidity is equally independent of inspissation, and coagulation, and is produced on the globules themselves, which, in fact, are the part which exhibit it. In accordance with this, when the viscid fluid is examined with the microscope, its globules have a somewhat fainter appearance, as if their transparency were increased; and they also appear a little expanded, and adhering together more or less, and if moved, when in the field of the microscope, some of them may be seen to be a little elongated.[1]

6. According to a recent analysis of pus by Dr. Gueterbock, 100 parts of this fluid from an abscess in a mamma, were found to consist of—

[1] In accordance with this view, it may be remarked that the time required for different varieties of pus to become viscid on admixture with solution of muriate of ammonia is far from constant; some exhibit the effect almost immediately; others not till after several hours.

86.1   Water.
1.6   Fatty matter, soluble only in hot alcohol.
4.3   Substances soluble in cold alcohol, viz. fatty matter and
       ozmazome.
7.4   Substances soluble neither in hot nor cold alcohol, viz.
       albumen, pyina, and the substances of the globules
       ("globuli et grana puris.")
0.6   Loss.

100.0[1]

From my own experiments I am disposed to consider these re-
sults as correct and deserving of confidence.   They have been con-
firmed generally in two or three instances, in which I have sub-
mitted pus of an abscess to analysis. The method of analysis which
I have followed has been somewhat different from that used by the
ingenious author just quoted.

First, The purulent fluid was slowly evaporated to dryness, at a
temperature below that of boiling water.

The yellow residue of a greasy feel, was then treated with di-
lute spirit of wine.   The solution thus obtained, evaporated, yielded
a matter of a light, bright yellow hue, soluble in water, and par-
tially deliquescent, consisting chiefly of saline matter and of ozma-
zome.

Next, pure alcohol was used, and heat employed, nearly to the
boiling point.   Whilst hot, the solution formed was decanted and
evaporated to a small bulk.   On cooling, a notable portion of fatty
matter was deposited, also of a light yellow hue.   The residual fluid
decanted and evaporated, yielded only a very little matter of a brown
hue, consisting chiefly of fatty matter, mixed, I believe, with a little
ozmazome.

The albuminous residue not dissolved either by dilute spirit of
wine, or by concentrated alcohol, was in large proportion, compared
with the quantity of the substances mentioned, extracted by the
spirituous menstrua.   The precise proportions of the constituent
parts, I have not attempted to verify; a great deal, I apprehend,
remains to be done, before this can be considered of much import-
ance.

The fatty matter, which I have obtained from pus has been sim-
ilar to that described by the author last quoted; of a butteraceous
consistence; moderately firm at 60°; semi-fluid at 80°; and liquid
at a somewhat higher temperature, according to Dr. Gneterbock at
about 140° F. and when pretty strongly heated giving off an ammo-
niacal smell.   Dr. Gneterbock mentions that he could obtain no
proof of the presence of cholestrine in it, which recently it has been
asserted exists in pus. This substance, he justly remarks, is depos-
ited from hot alcohol in cooling, in the form of crystalline plates:

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[1] De Pure et Granulatione, p. 17.—Berol. 1837.

no such plates appeared in any of the processes with alcohol, to
which pus was submitted in his experiments; and I have the same
remark to make on mine; thus indicating the absence of cholestrine.
Probably the fatty matter is a mixture of two or more kinds, one
capable of combining with alkalies, and forming a soap; another
not, and so resembling and approximating to cholestrine.   Dr. Gue-
terbock appears to be of opinion, that the fatty matter of pus is free
adipose matter mixed with the purulent fluid, inasmuch as adipose
globules may be occasionally detected in it with the microscope,
and the paper used in filtering pus is rendered slightly translucent.

This conclusion does not appear to me very probable, or well
supported; it seems to me more likely, that the fatty matter belongs
essentially to pus, and enters into the composition of the pus-glo-
bule, and that it is to it that the pus owes its colour and its cream-
like character.   In an instance of phthisis with much emaciation, I
have detected it in the pus of a vomica; in which it could hardly be
expected to be found, unless essential.

Of the substances inferred to exist in pus and not soluble in alcohol,
that which has been called pyina is the only one on which I shall
offer any remark.   Dr. Gueterbock, the discoverer of it in pus, and
by whom it has been named, considers its characters well marked
as a proximate principle; namely, solubility in water, insolubility in
alcohol, and acetic acid, precipitation from solution in water by this
acid, and non-conversion into gelatine by long continued boiling.   I
would not wish to appear unduly sceptical; but I cannot refrain
from doubt relative to the positive existence of this substance, inde-
pendent of the means employed to procure it.   It is possible that it
may be formed by the action of acetic acid on the albuminous part
of the pus.   I have stated at page 53 of this volume, that the serum
of the blood is not coagulated by strong acetic acid.   I have since
found, that although there is no effect immediately produced, nor
for many hours, yet that there is in the course of many days, and
that if the mixture of the serum and acid be kept, after a time it will
be found jellied or feebly coagulated; and farther, that the gelati-
nous compound under the microscope exhibits very minute glo-
bules.   Now, is it not possible that this acid on the serous fluid of
pus may have a more rapid effect, and produce instantly a substance,
a form of albumen, such as that which has been called pyina?   The
properties of animal and vegetable substances are so easily altered,
by feeble agencies, sometimes with, sometimes without, new arrange-
ments of their elements, independent of any alteration in their pro-
portions, that great caution requires to be observed relative to the
adoption of new proximate principles.

7.—Dr. Gueterbock has given a very good account of the pus-
particles, under microscopical examination, with which my obser-
vations perfectly accord.   After remarking that they vary in size,
and that hitherto those of the greatest magnitude only had been de-
scribed by authors, he adds,—" Corpuscula majora formam habent
pæne rotundam, et sphæricam, quam, si aqua evaporante magna

natant celeritate, luculenter videre potes.   Et forma et magnitudine
paulo discrepant, alia (et major quidem pars) satis rotunda, alia
irregularia, marginibus aliorum acutis, aliorum crenatis, superficie
in plerisque rugata, mororum non dissimilis, ita ut, qui primum ea
videant, alia granula minora supersedere credat."[1]   The same writer
has also described faithfully the appearance of the individual pus-
globule, when carefully observed and subjected to the action of dif-
ferent agents.   The result of his observations is that the globule is
composed of two parts,—an involucrum, which is rendered more
transparent by water, spirit of wine and acetic acid,—and of cen-
tral granules, on which the agents mentioned have not the same
effect ; and, in consequence, they are rendered by them, more con-
spicuous.

Mr. Gulliver, too, has recently directed his attention to the same
subject, and, quite independently, has arrived at the same conclu-
sion relative to the compound nature of the pus-globule.   This gen-
tleman considers the central granules, or, as he designates them,
molecules, as composed of a peculiar principle, possessed of proper-
ties similar to those belonging to pyina.[2]

Of the accuracy of the observations generally, on which this con-
clusion is founded, I have not the slightest doubt.   But I am not
quite free from doubt concerning the inference, viz. that the cen-
tral granules or molecules, are composed of a distinct substance.
Fibrin is not dissolved by acetic acid ; the spermatic entozoa are not
dissolved by it ; coagulated serum is not dissolved by it ;[3] nor is fatty
matter ; in brief, its solvent power over animal substances generally
is very inconsiderable, as I have briefly mentioned, when treating of
the action of distilled vinegar.[4]   I have sometimes imagined that

[1] When pus is mixed largely with water, and thrown on a filter, the greater
part of it is retained on the filter ; a small portion passes through with the
water ; under the microscope, the particles of the latter appear more irregular
in form, and most of them smaller than those of the former.   Also, when
largely diluted with water, in a tall jar, and set aside at rest, a difference
will be found in the time of subsidence of the particles, some falling down
immediately, others not for many hours ; and these last, on microscopical
examination, I have commonly found ill defined in form, and of a looser
texture.

[2] Vide London Medical Gazette, May, 1839, for a notice of Mr. Gulliver's
paper, " On the composition and elementary structure of the Pus-globule."

[3] Contrary to what Dr. Gueterbock mentions, I have not found the involu-
crum of the pus-globules dissolved by acetic acid ; only rendered more trans-
parent.   If, after the action of this acid, an alkali be added to neutralise it,
or if a solution of common salt be mixed with it, to change the refractive
power of the medium, the involucrum is rendered discernible.

[4] Doubt, perhaps, similar to that which I have expressed, has occurred to
Dr. Gueterbock ; this I infer from his remark relative to the composition of
pus-globules ; that he cannot venture to determine whether they consist en-
tirely, or only in part of fibrin ; and also from the notes to this remark, con-
cerning the action of acetic acid :—" Berzelio duce fibrinam sanguinis bovini
et albumen ovi calore coagulatum bene abluta acido acetico concentrato
superfudi, ut utrumque intumesceret, at aqua coquente tantum vix utrius-
que solutum est, ut solutione kali ferroso-hydrocianici parcum fieret sedi-

the appearance attributed to central molecules might be owing to par-
ticles of air, disengaged by the action of the acid on the minute
quantity of carbonated alkali, which there is reason to believe exists
in the pus-globules.   Some facts are not unfavourable to this idea;
thus, though the globules are rendered more translucent by a solu-
tion of potash, soda, or ammonia, the molecules are less apparent
than even when in water, and if either of these alkalies be added to
a mixture of pus and acetic acid, the molecules, before very visible,
become indistinct.   At other times, I have supposed that the ap-
pearance in question might be connected with the existence of par-
ticles of fat in the globules, on which the acetic acid would have
little effect, and might render more conspicuous by increasing the
transparency of the enveloping part.   The circumstance that alco-
hol does not render the molecules more but less distinct, whilst it
augments the translucency of the involucra, is rather favourable to
this idea.   I venture to express the foregoing doubts, and to offer
these conjectures on this obscure subject, with hesitation, and the
hope that they may lead to further inquiry.   In justice to Mr. Gul-
liver, I should remark, that I have not read his paper referred to on
the composition of the pus-globule; the volume of the Transactions
of the Medico-Chirurgical Society, in which it will appear, not hav-
ing yet been published.

8. Relative to that important question in pathology, the mode of
formation of pus, it is matter of very interesting inquiry, whether it
is a secretion depending on a peculiar state of vessels in the part in
which the fluid is collected, or whether it may not be formed in the
general mass of the circulating blood, and be poured out merely
where it is found collected.   The phenomena of ulcers are favour-
able to the first view ; and seem at least to prove, that in certain
cases its origin is local, and that it is formed in the part where it
presents itself.   On the other hand, the collections of pus which so
often occur in erysipelas in deeply seated parts, remote from the
cutaneous inflammation, and without symptoms of corresponding
local inflammation, favour the latter view.   This view is also fa-
voured by the detection of globules resembling pus-globules in the
blood during the process of suppuration,—whether in instances of
superficial suppuration, as in small-pox, or in deeply seated, as in
tubercular phthisis, or lumbar abscess ; and the analogy which there
is in a certain degree between the puriloid matter, occasionally
found in masses of fibrin, in the cavities of the heart and of the great
vessels, seems to me likewise to support it.   Inquiry now is happily
directed to pyogenesis, and with the aids which the collateral sci-
ences are capable of affording, there is good ground for hope, that

mentum. Miratus, quod Berzelio teste in omnibus libris chemicis adnotatum
est, et fibrinam et albumen acido acetico solvi, experimentum sæpius repe-
tivi, nonnunquam acido acetico diluto adhibito sed semper idem fuit eventus."
He justly adds, " Si hæc experimenta probantur, novos in tenebras et confu-
siones incidimus, et ad novam materierum illorum simplicium disquisitionem
excitamur."

its termination will be successful; and if successful, that it may be the commencement of a new era in pathology.

XIX.

AN ACCOUNT OF SOME EXPERIMENTS AND OBSERVA- TIONS ON THE TORPEDO.

The extraordinary property which the torpedo possesses of im- parting shocks similar, as far as sensation is concerned, to those of the Leyden vial, was one of the last subjects to which the attention of my brother, the late Sir Humphry Davy, was given. In the memoir which I have published of his life, particulars are related, showing how great was the interest he took in the inquiry, and how anxious he was that it should be prosecuted—and how he urged me to enter on it. The last letter he ever addressed to me, written from dictation when he considered himself dying, is strongly ex- pressive of his feelings. I shall insert it—partly as an expression of the feelings, and partly as showing what he considered one of the chief desiderata in the inquiry.

Rome, Feb. 25th, 1829.

" My dear John,

" If I had not had this attack, it was my intention to have gone to Fumicina, or Civita Vecchia, to make some experiments on the torpedo. I hope you will take up this subject, which, both as a comparative anatomist and chemist, you are very capable to eluci- date. You will see my paper on the torpedo in the manuscript book which I have left in Mr. Tobin's hands.¹ It was my wish to have exposed an unmagnetised needle to the continued shocks of a tor- pedo in a metallic spiral, making the metallic communication per- fect with both electrical organs. There is in my little box an appa- ratus which I hope you will use. Large living torpedos may be procured at Fumicina or Civita Vecchia. The shock from a very small jar will make a needle magnetic, provided it is entirely passed through the metallic conductors; but I did not find this effect when there was any interruption by water. There are many things worth attending to in the two kinds of torpedinal fishes found here—the tremula and occhiatella. Pray do not neglect this subject, which I leave to you as another legacy. God bless you, my dear brother, your affectionate friend,

" H. DAVY."

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¹ This, his last paper and communication to the Royal Society, was pub- lished in the Philosophical Transactions for 1829; it was written in the preceding October; its results were entirely negative.

The researches which I instituted in compliance with these his wishes, I had the satisfaction of commencing at Rome, in his presence, and of thinking that they afforded him amusement in his last days; they were continued in Malta, and finished in England. The experiments on the living fish were entirely made in Malta, under very favourable circumstances: the anatomical part of the inquiry was commenced at Rome. Most of the results have already been published in the Philosophical Transactions, in two papers— the first of which was read at the Royal Society in March, 1832, and the second in June, 1834. I may premise that when I entered on the inquiry, it had not been ascertained that the electricity of the torpedo, considering its peculiar influence to be electrical, had either the power of acting electro-chemically, in separating the elements of any compound bodies; or magnetically, either in affecting the needle in the multiplier, or in imparting magnetism to iron; or lastly, of generating or producing heat: points to which my experiments were particularly directed, and with positive and successful results. I may further remark, that in describing the results obtained, I shall not strictly follow the same order as was observed in my first publication, or restrict myself entirely to the details given in them: what in those were necessarily disjointed, will here be incorporated; and I shall make without hesitation such alterations as any additional knowledge I have since been able to attain by my own labours may seem to render advisable.

1.—*Experiments on the electricity of the torpedo.*

From the preceding letter, it appears how desirous my brother was of trying the effect of the shock of the torpedo on a needle placed in a spiral wire. The result, he was of opinion, would be conclusive as to the nature of its electricity—that is, whether it should be considered as distinct, and of a peculiar kind, or merely a variety of common electricity, or at least analogous to kinds already known.

Anxious to make this trial, I had an apparatus in readiness, which, with common electricity, I had found to answer extremely well. It consisted of a fine copper spiral wire, (the spring of a bracer,) about one inch and a half long, and one tenth of an inch in diameter, containing about one hundred and eighty convolutions, and weighing about four grains and a half. This was inserted into a glass tube, just large enough to receive it, and secured by corks.[1] The wire passed through the cork at each end, and was connected with strong wires with glass handles for the purpose of contact. The wire intended to be applied to the under surface of the fish was one twenty-fifth of an inch in diameter; that intended for the upper surface was stiffer, being one fourteenth of an inch in

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[1] The apparatus referred to by my brother, in his letter, did not answer so well: the spiral was of silver wire, wound round a glass tube.

diameter; and its greater strength was useful, as it was necessary to employ it occasionally with some force to rouse the fish when averse to give a shock. The first trial I had an opportunity of making with this apparatus was successful. It was on the 3d of September, 1831, at eleven o'clock at night.[1] The fish used was a small one, about six inches long; it had been just caught in a hand-net, and immediately put into salt water, and was very active. A needle, perfectly free from magnetism, was introduced into the spiral, and there confined by the corks, and the spiral was carefully connected with the insulated wires for contact. The fish for the experiment was placed in a glass basin, and was barely covered with water. One wire was applied to the under surface of the electrical organ, and the other to its upper surface, and contacts were made at intervals during about five minutes, when the fish seemed much exhausted by its exertions. On taking the needle out, and bringing it near some fine iron filings, it was found to be magnetic, and powerfully attracted them. This experiment I have repeated many times with fishes of different sizes, some larger and some smaller, and with the same result whenever the fish has been active, and the contacts similarly made.

The next trial instituted on the electricity of the torpedo was on the multiplier; it was one which belonged to my brother, the needle was poised on a pivot, and was not very sensible. The precaution was taken to insulate the instrument well, by smearing with sealing wax the feet of the stand supporting the coil. The same wires for contact were used in this as in the former experiments, and the junctions were carefully made. Applying one wire to the under surface, and the other to the upper surface, using the fish first mentioned, and after an interval of only two hours, I succeeded in obtaining decisive results; the first shock had a powerful effect, the needle made half a revolution: and other trials were in accordance. The needle, by active fishes, was generally thrown into violent motion, occasionally describing nearly a circle, and even by the feeblest it was distinctly affected. I have met with no instance of a fish which had the power of magnetising a needle in the spiral wire failing to move the needle in the multiplier; but I have met with more than one example of a fish whose electricity was equal to the latter effect and not to the former.

I have not mentioned, in my paper in the Philosophical Transactions, that I made comparative trials with the same instruments on the salt water alone by contact. Referring to my notes, I find this distinctly mentioned, and that no effects were perceptible; neither the needle in the spiral acquired the slightest degree of

[1] It may appear singular that this experiment was not made before. The explanation is easily made.—On my return from Rome to Malta, in June, 1829, I was assured that the torpedo is not known in the latter place. It was not until the summer of 1831 that I found out that I had been misinformed, and that, with a little trouble, the fish may be procured alive at all seasons of the year.

magnetic power, nor did the needle of the multiplier experience
any divergence: fully satisfying me that the effects were owing to
the peculiar power of the fish. Indeed, that this power was the
cause, was manifest from the variability of the effects, in accordance
in degree of strength with the energy of the torpedo, as indicated
electrically by the sensation which its shock imparted.

The experiments which I have instituted, with a view to ascer-
tain if the electricity of the torpedo has any igniting power, or
power of passing through air, and of producing light, have been
attended with less satisfactory results. Very active fishes were tried
on circles of perfect conductors, interrupted only by a space just
visible with the aid of a powerful magnifier. The terminal wires,
coated with sealing wax, excepting at their extremities, were intro-
duced through a perforated glass stopple into a small glass globe,
which was held in the hand of an assistant. The contacts were
made in the dark; but not the faintest spark could be perceived,
nor could any ignition be perceived when the extreme points were
connected with silver wire, not exceeding one thousandth of an
inch in diameter.

When a torpedo was put into a metallic vessel, insulated by a
glass stand, and contacts were made on its back with the insulated
wire resting on the edge of the vessel, or at a distance from it, lumi-
nous appearances were frequently produced, sometimes in the form
of sparks, and sometimes in the form of flashes, not unlike summer
lightning on an infinitely minute scale. At first, I was disposed to
consider the phenomena electrical; but, on reflection, it occurred
that they might depend on the presence of animalcules, which be-
came luminous when agitated. And this, I believe, is the correct
explanation of the effects; for when the salt water was agitated
without the torpedo, sparks of light now and then were seen, and
the flashes or corruscations might have been owing either to lumi-
nous matter thrown off from the surface of the fish when it gave
the shock, or to the shock simultaneously stimulating several par-
ticles, which in consequence shone for an instant.

The only positive result which I have obtained on the passage
of the electricity of the torpedo through air, has been by using a
chain as a substitute for a wire of communication. It was a small
gold chain, composed of sixty double links, each circular, and
about one tenth of an inch in diameter, fastened, unstretched, to a
dry glass rod at each end. Holding the upper portion of this chain
in one hand, and the under wire in the other, (the hands being
moistened,) I irritated by means of them the upper and under sur-
face of an active fish; the shock which it gave was pretty strong,
reaching beyond the fingers, and was felt with the same degree of
force in both hands. This seems to show that the air is not im-
permeable to the electricity of the torpedo; and the same conclusion
may perhaps be drawn from the facility with which I have found
it to pass through a circuit of wire in which there have been no

less than seven joinings, and those made merely with ordinary care with the fingers, without the aid of any instrument.

In accordance with Mr. Walsh and my brother, I have in no instance seen the torpedo affect the electrometer, or exhibit any the slightest indication of a power of attraction and repulsion in air.

It having been stated on high authority that a spark has been obtained from the gymnotus electricus,[1] I thought it right, after obtaining the preceding results which were detailed in my first paper—to renew the attempts to procure a spark from the torpedo. I have tried the method which it is said succeeded with Mr. Walsh in the instance of the gymnotus, namely, dividing with a penknife gold leaf attached to glass, and connecting the divided parts with the contact wires. Using an active fish in this way, I could neither observe a spark in the dark, nor in the light detect the slightest indications of the passage of electricity, either by the galvanometer, or the test of the sensation or shock. I have been equally unsuccessful in using an electroscope, formed on the principle of Coulomb's, which displayed sparks when touched either by a small rod of glass slightly excited, or of sealing-wax;—even when the torpedo was taken out of water, and all loosely adhering moisture removed, no effect could be obtained, not even the slightest indications of attraction. I have varied the trials, using highly rarefied air at ordinary temperatures, and also condensed air deprived of moisture, with the same negative result. And I have been equally unsuccessful in substituting flame; unless the metallic points were in contact in the flame of the spirit-lamp, the passage of the electricity appeared to be completely interrupted. In very many experiments, employing the most active fish, if there were any visible space between the ends of the red-hot platina wire, I never witnessed the galvanometer in connection with one wire affected, nor could obtain a shock. Reasoning on the subject, this perhaps is what might be expected, considering that the surface of the fish is a better conductor than air. One fact, however, which I had observed, afforded some encouragement to persist in the trials—the fact that the torpedinal electricity passes through distilled water, which is a worse conductor of it than its own skin.

I thought it possible, that by insulating the torpedo on a plate of dry glass, and wiping its circumference dry, and smearing it with oil, that the galvanometer might be affected. But in this, too, I have been disappointed; not even in flame, when the interruption

[1] Mr. Walsh is said to have written to M. Le Roi to the above effect; and also that Sir John Pringle and M. Magellan assured M. Le Roi they had witnessed the result repeatedly. Vide Bloch's Ichthyologie, p. 1020. Bloch refers for his information to Rozier's Journal, Ann. 1774. M. De Humboldt (Annales de Chemie et de Physique, tom. xi. 427,) states that the same result has been observed by M. Fahlberg. He refers to Vetensk Acad. ny. quart. 2. (1801.)

of the circle has been only just visible, has any effect on the instrument been produced.[1]

Mr. Farady, in the third series of his Experimental Researches
on Electricity, states, that he has little or no doubt, were Harris's
electrometer applied to the torpedo, the evolution of heat would be
observed.[2] I have made very many experiments on this subject,
completely establishing Mr. Farady's anticipation.   The instrument employed was similar to that described by Mr. Harris in the
Philosophical Transactions for 1827, differing merely in the wire
passed through the small globe being exceedingly fine, and of platina, formed after Dr. Wollaston's method;[3] in having a small
stop-cock for regulating the height of the spirit in the stem; and in
using as small a quantity of spirit as possible.   The small globe or
bulb of the thermometer was defended from the variable temperature of the surrounding air, by being included in a wooden box.
The delicacy of this instrument was so great, that the spirit in the
stem was not only moved by a single spark of the electrical machine, but even very distinctly by the electricity of a single voltaic
combination, composed of a copper and zinc wire, the former 1-25th
of an inch in diameter, the latter 1-50th, excited by dilute sulphuric
acid.

This instrument was strongly affected by active fish, and even
distinctly by weak ones;—indeed, occasionally, when it formed part
of a circle in connection with a galvanometer, I have seen it affected alone, the galvanometer affording no indication of the passage of
the electricity.   Using two air-thermometers of the same construction, each connected with the wires for contact at one end, and with
the galvanometer at the other, the heating effect of the electricity
of the torpedo has been apparently diminished, and even more distinctly diminished on adding to the circle another link of very fine
platina wire.   And at the same time its influence on the galvanometer has been diminished, and its power of imparting permanent
magnetism to a needle placed in a spiral, both forming part of the

[1] Since the above remarks were first made, M. Matteucci has obtained
sparks from the torpedo, and M. Faraday from the gymnotus--not indeed
directly, as Mr. Walsh was supposed to have done, but by means of a
"magneto-electric coil," the object of which, it has been hypothetically said,
is to prevent the electrical principles from neutralising themselves directly
through the conducting matter adjoining ; and to force them to re-unite at a
distance, by traversing the thin stratum of air in which the spark was taken.
Bibliothèque Universelle de Genève, June, 1836, p. 387.   The preceding
part of this note was written in December;—I have since learned (now January 26, 1839,) that a spark has been taken direct from the gymnotus, in the
Adelaide Gallery, and that it was accidentally observed in making an experiment instituted with a different object in view.   My authority for this
singular occurrence, and one so difficult of explanation, is the very intelligent superintendent, Mr. Bradley.

[2] Philosophical Transactions, 1833, p. 46.

[3] Philosphical Transactions, 1813, p. 114.

circle. When heat has been applied to the entire link of platina by means of a spirit lamp, so as to render it red hot, the diminution of effect disappeared; and equally so, as well as I could judge from many experiments, whether acting on the thermometer, the galvanometer, or the needle in the spiral.

It appeared not improbable that a short portion of very fine platina wire might be ignited in the passage of the electricity of the torpedo. I have made several experiments to ascertain this, but I have never witnessed the effect, even in perfect darkness, and using fish the discharge of whose electricity at the same time converted a needle into a tolerably powerful magnet; the needle having been put into a spiral connected with the fine wire, so as to afford a test of the strength of the electricity. This want of ignition may at first view seem contrary to the effect on the thermometer; but, perhaps, it ought not to be considered so, taking into account the rapid manner in which the heat evolved in the fine platina wire must be carried off by the adjoining compound wire of platina and silver.

The experiments which I have made on the electricity of the torpedo as a chemical agent, have been of a satisfactory kind. A small glass globe, of the capacity of about half a cubic inch, was used for holding the fluid to be acted on; and fine wires communicating with the contact-wires were introduced into it through a perforated glass stopple, and they were cemented with sealing-wax along their whole course in the vessel, excepting at their points. By means of this little apparatus, I first tried the effect of a small active fish on a strong solution of common salt; the terminal wires were of silver. The contacts were made on the upper and under surface of the fish in the usual manner; minute bubbles of air collected round the point communicating with the under wire, but none at the other point. After an interval of some hours, fine gold wires were substituted for the silver wires: now gas was evolved from each extremity, but in largest proportion and in smallest bubbles from the point connected with the under wire.

The next experiment was made on a strong solution of nitrate of silver; the terminal wires were of gold, the effect was distinct; the extremity of the under gold wire became black, and only two or three bubbles of air arose from it; the extremity of the upper gold wire remained bright, and it was surrounded with many bubbles of air. A similar experiment was made on a strong solution of superacetate of lead, and with results which were similar; but the effects appeared to be produced with greater difficulty; they were not distinct till the fish had been much irritated and seemed to put forth all its energy.

These electro-chemical experiments on the torpedo were some of the earliest which I instituted on its electricity; the notes of the first successful one bear the date of the 9th of September, 1831. In the autumn and early winter of 1833, I instituted several other trials, which were amply confirmatory of the preceding. In these latter experiments I did not, as in the former, coat with sealing-wax

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the wires introduced into the fluids, leaving the naked points only
exposed. Though the wires were naked, and in every instance in-
troduced more than a quarter of an inch into the fluid, and the dis-
tance was at least a tenth of an inch, yet satisfactory results were
obtained. Using either a saturated solution of common salt, or a
mixture of equal parts of sulphuric acid of commerce, and water,
and platina or gold wires, gas was given off round each wire under
the influence of the discharge of the electricity of an active fish, one
contact wire being applied to the under surface and the other to the
upper surface of the torpedo. When steel needles were used with
the salt water, then gas was disengaged only from the one in con-
nection with the under surface of the fish, the other needle becom-
ing oxidated. Using a strong solution of nitrate of silver, and gold
wires, silver was precipitated only on that in connection with the
under surface; employing strong nitric acid and platina wires, gas
was given off from one only, that in connection with the upper sur-
face; using a solution of iodide of potassium and starch,[1] the iodine
in combination with the starch, as indicated by the discoloration,
was precipitated round the same wire.

Even the decomposition of water has been effected, when the
circle has been interrupted by four portions of the solution of com-
mon salt, contained in small tubes, with two needles in each, the
needles in one connected with those in the other, and at the same
time with the galvanometer,—a spiral holding an unmagnetised
needle, and an air thermometer. And simultaneous with the che-
mical decomposition, the needle in the galvanometer has been
moved, and the spirit in the air thermometer has been raised, and
the needle in the spiral has been magnetised.

Mr. Walsh, from his experiments, inferred that the two sides of
the torpedo are, when in action, in opposite electrical states.[2] The
results just described appear to prove that its under surface corre-
sponds to the copper or negative extremity of a voltaic battery, and
its upper surface to the zinc or positive extremity.[3]

To ascertain, if they preserve the same relation to each other,
when the fish is made to act on the multiplier, and on the needle
in the spiral, the following experiments were made. Successively
at different times, with the same fish, and also with different torpe-

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[1] When starch, in powder, is added to a saturated, or nearly saturated,
solution of the iodide of potassium, a transparent gelatinous mass is formed.
This I have used in my experiments; a single combination of copper and
zinc wire, acted on by very dilute acid, occasions, in this compound, a pre-
cipitation of iodine. The iodine is precipitated round the negative wire, or
that connected with the copper. The effect is the same, whether platina or
silver is made to convey the electricity into the saline solution. The decom-
position probably is the effect of the hydrogen disengaged; it is not an im-
mediate electro-chemical effect.

[2] Philosophical Transactions, abridged, vol. xiii. p. 475.

[3] In my first paper on the torpedo, a mistake was inadvertently made rela-
tive to the electricity of the opposite surfaces of the torpedo, which has been
pointed out by M. Becquerel, in his very able work on electricity.

dos, comparative experiments were tried on the course of the needle
in the multiplier, when affected by the electricity of the fish, and by
that of a couple of very small plates of copper and zinc immersed
in a weak acid. In every instance the wire communicating with
the under surface of the torpedo, was found to correspond in its
effect with the copper plate, and that with the upper surface with
the zinc plate; and whether one wire was in communication with
the under surface of the fish, and the other with the upper, or the
former with the copper plate, and the latter with the zinc plate, the
deviation of the needle was in the same direction; its south pole
turned to the west, and of course, its north to the east; and if the
lower contact wire was made the upper, the effect on the deviation
of the needle was identical with a change of the plates.

I have found the same uniformity of result in the polarity im-
parted by the torpedo to a needle in the spiral wire; the extremity
of it, nearest the under surface in the circle, has always acquired
southern polarity, and the other extremity of course northern.

Besides the preceding results, I have obtained some others, of a
miscellaneous nature, which, in consideration of the obscurity of the
subject, and its novelty in many points, I do not think it right to
withhold. By connecting the spiral with the multiplier, and charg-
ing the former with as many small needles as it could hold, namely,
eight, I ascertained that a single discharge of the electricity of an
active fish moved the needle in the multiplier powerfully, and con-
verted all the needles into magnets; and each of them, I believe,
was as strong as if one only had been used.

Using two spirals charged with needles, one connected with one
end of the multiplier, and the other with the other end, the effects of
the discharge were similar to the preceding, both on the needle of
the multiplier, and on the needles in the spirals. In two instances
the needles in the spiral connected with the upper surface, were
most powerfully magnetised; and in one instance, the effect was
greatest on the needles in the lower spiral. In this last instance,
nine needles were acted on in the under spiral, and six in the upper:
the fish which produced the effect was, with one exception, the
smallest that I had ever used.

In a few experiments on metallic conductors, the effect of the
electricity of the torpedo on the galvanometer appeared to be much
the same, whatever metals were used, and whether rusty or bright,
provided the junctions were bright. The mass of metal appeared
to have more influence; the effect, as might be expected, diminished
with the increase of the mass; thus, when a poker weighing about
two pounds formed part of the circle, the effect on the electrometer,
though distinct, was less powerful than when it was omitted; and
when a large copper coal-scuttle was substituted for it, the effect
was still more diminished, the deviation of the needle being only
just visible. Extension of surface, as in the instance of increased
length of wire, had a sensible modifying effect: thus, in an experi-
ment in which about a thousand feet of wire were used (formed of

three pieces, two about one-fiftieth of an inch in diameter, the third piece considerably finer), the motion of the needle was decidedly slower than when a short length of wire was employed, though the space traversed by the needle was not perceptibly different.   I shall notice only a few of the experiments which I have made on imperfect conductors.

When I have held the contact-wire in the palm of each hand wetted with salt water, and have touched with the fore-fingers the upper and under surface of a torpedo, I have felt its shock distinctly; but in no instance when the multiplier has been connected with the wires has it been affected; and when the spirals have been connected with them I have once only seen the needles in them converted into magnets.   This effect accompanied a very smart shock from a young active fish, about six inches long, just taken.

When the touching ends of the contact wires have been covered with leather soaked in salt water, or with cotton thread, all the effects of the fish, as might be expected, were witnessed, as if these imperfect conductors had not intervened; the shock was felt by the hands holding the wires, needles in the spirals were magnetised, and the multiplier was moved.

When a cotton thread, soaked in salt water, or in a strong solution of salt, was interposed beyond the contact wires, both the power of affecting the multiplier and of giving polarity to the needle in the spiral, was arrested and this was uniformly the result in a considerable number of experiments made with three different fishes, of which two were very active, and with perfect conductors free from this interruption, produced both effects readily.   But the power of giving a shock was not equally arrested; for on removing the multiplier and spirals, and holding with the wet fingers the wires attached to the moist cotton thread, the shock was several times distinctly felt on stimulating the fish.   The space of cotton thread between the wires was about one-tenth of an inch, and to secure its perfect humidity or wetness, it was enclosed in a glass tube, with corks at each end, through which the wires passed.

When the apparatus, already described in noticing the chemical effects of the torpedo, was substituted for the wet cotton thread, the tubes being filled with a strong solution of salt, the multiplier was affected, and gas was given off at each of the points of the gold wires; and when steel needles were used, a free current of gas rose from the point connected with the under contact wire, and not a particle from the other point.   In these experiments, there were interposed at the same time the chemical apparatus, one on each side; the spiral, one also on each side; and the multiplier intermediate; and there were necessarily many junctions of wires.   And in an after experiment, in addition to these, an air thermometer (Harris's electrometer) was introduced, and very distinctly affected.   I scarcely need add, that in an experiment made expressly to ascertain this point, the shock of the fish was felt beyond the saline solution; for it had been previously proved by the experiments of Mr.

Walsh, that salt water, even in a long circuit of imperfect conductors, has a power of transmitting the electricity of the torpedo.

The tests or indications of the electricity of the torpedo, at present known, are six in number; namely, the physiological effect, as the sensation it imparts is sometimes called; the chemical effects, as the precipitation of iodine, the decomposition of water, &c.; its effects on the thermometer, on the galvanometer, and on steel in the spiral. These different tests, in point of delicacy, I am inclined to believe, are in the order in which they are enumerated. That the two first should be placed highest, and that sensation should have the precedence, the experiments which I have made appear to prove independently of all analogy.

When the human body has formed part of a circle of communication between the two opposite surfaces of a torpedo, and also a chemical apparatus with platina wires and the solution of iodide of potassium and starch, the shock experienced by the hands has been strong, and the chemical effect either null or slight; no gas appearing when a strong solution of salt has been used, and no precipitation of iodine occurring unless the platina wires were very nearly in contact, and the fish energetic.

When, besides the human body, and the chemical apparatus, the galvanometer has been introduced into the circle with the air thermometer and the spiral, the shock has been experienced as if it had been received direct from the fish; but I have never witnessed at the same time any other effect.

Not taking the human body into the circle, in trials on fish of very feeble electricity, I have witnessed the precipitation of iodine, when neither the air thermometer, nor a delicate galvanometer with a double needle has been affected.

The same kind of evidence has been obtained of the thermometrical test, being next in point of delicacy, inasmuch as I have seen the air thermometer affected by a fish which had no influence on the galvanometer, in connection with the wire of the thermometer.

That the needle in the spiral is the least delicate test does not require to be insisted on. The electricity of a torpedo, of almost feeble energy, has been equal to produce all the effects alluded to at once, excepting sensation, as already explained, and excepting the imparting permanent magnetism to the steel needle. The last effect, as might be expected, has required greater force; a moderate force, however, has been sufficient when a very slender needle has been used, and a spiral of fine wire closely coiled, only just capable of receiving the needle.[1]

The few experiments which I have made on the torpedo, analogous to those instituted by Mr. Todd, described by him in the Philosophical Transactions for 1816, have afforded very similar results. When the brain has been divided longitudinally, the fish has con-

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[1] The spiral wire above alluded to, is fine copper wire guilt, having about 300 convolutions to the inch; an inch of it weighed four-tenths of a grain.

tinned to give shocks. When the brain has been entirely extracted,
the fish instantly lost this power, though the muscles generally con-
tinued to act powerfully;—nor could any shock be procured in this
instance, either by puncturing with a sharp instrument the electri-
cal nerves, where they quit the cavity of the cranium, or where they
enter the electrical organs, just after passing between the branchial
cartilages.   On one occasion, however, it may be mentioned, that
when a small portion of the brain was accidentally left, contiguous
to the electrical nerves of one side, and with which they were con-
nected, then the fish, on being irritated (the remaining portion of
brain was touched) gave a shock to an assistant who grasped the
corresponding electrical organ.

M. De Humboldt states, that the shock of the torpedo may be pro-
cured by touching with the finger or hand, one surface only of the
fish.[1] The experiments which I have made expressly on this point
have led me to a different conclusion, namely, that it is requisite to
touch the opposite surfaces of the electrical organs or organ, or a con-
ductor or conductors connected with them, to receive a shock.   In
very many instances that I have irritated torpedos by pressing with
the finger on different parts of the back, as the upper surface of the
electrical organs, and on the margin of the pectoral fins, however
much the fish were irritated, I never had any sensation excited by
the electricity which there was reason to believe was discharged;
though immediately after, on touching the two surfaces, irritating
only the upper, shocks were received.   On some few occasions, I
have perceived a shock, when apparently only one surface of the
fish was touched; but I believe in those instances the discharge
took place through the water.[2]   In corroboration I may mention,
that in experiments in which one surface only has been touched
and irritated, the fish itself appears to make an effort to bring, by
muscular exertion, the border of the under surface (the upper being
pressed on) in contact with the offending body; and this I have wit-
nessed as distinctly in the fœtal fish as in the adult; clearly showing
that the effort is instinctive.   The conductor, which I suppose to
be necessary for conveying the electricity, when a shock is felt
without immediate contact, exists in salt water.   The galvanometer
has been affected when the two extremities of it have been brought
in contact, one with the back of the fish, and the other with the
water, two or three inches from the fish.   And, in one instance I
experienced a shock, although I touched the water alone, close to a
torpedo; it was in removing an active fish, by means of an earthen-
ware dish, from one vessel into another; the hand that received the
shock grasped the wet margin of the dish just as the torpedo entered
it.   I believe that the torpedo has the power of discharging its elec-

[1] Annales de Chimie et de Physique, tom. xi. p. 430.
[2] The most remarkable example of the kind of which I have any note, was
that of a young torpedo, which gave slight shocks to the hand on which it
was supported, whether just under the surface of the water, or just after it
was taken out of the water into the air.

tricity in any direction it chooses. This inference is drawn from finding that when one hand, in contact with the opposite surfaces of the fish, is receiving shocks, the other hand immersed in the water close by, has received no shock. And, in confirmation of this, I may mention (and at the same time to show how the discharge is connected with the volition of the animal,) that when I have applied to the opposite surfaces of a torpedo copper plates, merely gently touching, joined together by a copper wire, and then irritated the fish with the contact wires in the usual manner, the galvanometer attached to the contact wires has been distinctly affected.

In my first communication to the Royal Society, it is conjectured, that the mucus with which the torpedo is lubricated, may be a conducting medium between the two opposite surfaces. This was an erroneous view. It may serve that purpose to the surfaces individually. Seeing the error theoretically, I was led to examine the margin of the fins, and they have appeared to me to have less mucus adhering to them than any other parts of the fish, as if intended partially to insulate the electrical organs.

In that communication also, I stated my inability to account for my brother's not obtaining any positive results in his experiments on the torpedo. After reconsidering the subject, I am disposed to think it might have been owing to his using large fish, without the means of ascertaining their electrical activity, excepting by the shock. And, we have seen, that when the human body forms part of the circle of communication with a galvanometer, the latter is not affected in the passage of the electricity producing the shock; which may serve to explain his not having witnessed any effect on the instrument at Trieste. As regards the electrical energies of large torpedos, nothing is more uncertain. There appears to be no relation between the muscular and electrical power. I have seen strong vivacious fishes, which made great muscular exertions in the water, almost or entirely destitute of electrical power; and, on the contrary, I have seen others languid and moribund, which have exerted considerable electrical power. Small fish are almost always active electrically, and they are greatly to be preferred as subjects for experiments: moreover, they have this advantage—they can be kept alive a much longer time.¹ Mr. Walsh noticed, in the fish on which he experimented at Rochelle and the Isle of Ré, a retraction of the eyes of the torpedo, at the instant it exercised its electrical function. This I have not witnessed in the torpedos of the Mediterranean; nor indeed have I been able to associate any visible sign, any apparent movement of the fish, with the electrical discharge.

¹ The torpedo should be kept in an earthenware vessel, not a *wooden* one, which exercises some noxious influence on the fish: the water (the purest sea-water that can be procured) should be changed daily; and the coolest place should be selected for it—where the sun never shines.

2.—*Observations on the electrical organs of the torpedo, and on some parts of its structure connected with them.*

The peculiar columnar appearance of the electrical organs of the torpedo, and their great proportional size, the vast proportion of nerve with which they are supplied, the manner in which the columns are sheathed in tendinous fibres, have been dwelt on by all inquirers who have paid any attention to this fish; but I am not acquainted with any attempt to ascertain by experiment what is the exact nature of the substance of these organs, or the peculiar structure of which they are composed.

When I have examined with a single lens, which magnifies more than two hundred times, a column of the electrical organ, it has not exhibited any regular structure; it has appeared as an homogeneous mass, with a few fibres passing into it in irregular directions, which were probably nervous fibres.[1]

The specific gravity of the electrical organ, in comparison with that of parts of the fish decidedly muscular, is very low; including the upper and under boundary of skin, I have found it 1·026 to water as 1·000. The specific gravity of a portion of the abdominal muscles of the same full grown fish was 1·058, and that of the thick strong muscles of the back close to the spine 1 065.

The loss of weight which the electrical organ sustains by drying, is greater than I have observed in any other part of the fish. I shall give the results of one trial; the statement will convey an idea of the bulk of the different parts of the torpedo, as well as of the proportion of solid matter which they contain. The subject of the experiment, procured fresh from the fish market at Rome, was eight inches long, and across the widest part five inches broad. Entire it weighed 2065 grains. It was carefully divided, and the different parts mentioned were found to weigh as follows in their moist state:—-

Spleen	-	-	-	-	-	5·5 grs.
Pancreas	-	-	-	-	-	5·0
Testes	-	-	-	-	-	3·0
Kidneys	-	-	-	-	-	8·0
A pale cream coloured oval body close to left kidney	-	-	-	-	-	0·25
A reddish oval glandular body attached to the large intestine[2]	-	-	-	-	0 5
Liver with gall-bladder and ducts		-		-	-	105·0

[1] Farther microscopic examination has confirmed the above observation. The fibres of the voluntary muscles, even after having been four years in spirits, exhibited the peculiar transverse striated appearance: no regularity of structure; nothing distinctly characteristic was visible in the tissue of the organ in question.

[2] This is found in all the cartilaginous fishes; it contains a cavity communicating with the intestine, and abounds in follicles; its function seems to be similar to that of the appendicula vermiformis of man.

Heart and trunk of pulmonary artery	-			-	-	3·0
Gills including branchial cartilages				-	-	53·0
Gullet	-	-	-	-	-	11·0
Stomach	-	-	-	..	-	65·0
Upper valvular intestine -		-	-	-	-	29·0
Lower intestine -		-	-	-	-	5·0
Electrical organs	-	-	-	-	-	302·0
Head separated at first vertebra	-		-	-	-	165·0
Thorax consisting of cartilaginous case and mus-						
cles with pectoral fins attached			-	-	-	670·0
Abdomen without its contents	-		-	-	-	440·0
Tail separated just below the anus			-	-	-	195·0

By exposure to the heat of boiling water for about sixteen hours, the different parts were completely dried; their total weight was reduced to 322 grains, so that they had lost by drying 84·5 per cent.

The electrical organs now weighed				-	-	22 grs.
Head	-	-	-	-	-	25
Thorax	-	-	-	-	-	93
Abdomen	-	-	-	-	-	53
Tail	-	-	-	-	-	36
Liver (abounding in oil)	-	-	-		-	43
Residue consisting of other organs, and extract of						
fluids which exuded during the drying			-		-	50

From the above loss of weight of the electrical organs in drying, they appear to consist of 7·28 matter *not evaporable* at 212 Fahrenheit, and of 92·72 water, taking it for granted that the loss sustained is owing merely to the evaporation of the aqueous part. I lay stress on matter not evaporable, because I believe that the solid contents of the moist organs are less, and that the water which they contain holds in solution various substances.

This solution may be obtained by cutting the electrical organs into small pieces, and placing them in a funnel. The fluid part slowly separates. What I have thus collected was slightly turbid, of a very light fawn colour, just perceptibly acrid; it did not change the colour of turmeric or litmus paper; a cloudiness was occasioned by dropping into it a solution of nitrate of silver, which was not completely redissolved by *aqua ammoniæ;* it was copiously precipitated by acetate of lead, and a cloudiness was occasioned in it by nitrate of barytes, and by corrosive sublimate. By evaporation it afforded a residue which deliquesced partially on exposure to a moist atmosphere, and had an acrid and a bitter saline taste. The exact proportion of this weak solution of animal and saline matters I have not ascertained, and indeed it would be very difficult to determine with any accuracy, for only a small portion separates spon-

taneously; and if pressure is used, the fibres are broken, and the fluid expressed is mixed with a pulpy matter.[1]

When the electrical organs of the torpedo are immersed in boiling water, they suddenly contract in all their dimensions, and the columns from pentagonal, which they generally are, become circular. In my early experiments at Rome, they were rendered firmer by immersion for a few minutes, and the columns appeared to be tolerably distinctly fibrous and laminated, bringing to recollection the structure of the pile of Zamboni. Latterly I have not witnessed this effect; in a few seconds the tendinous fibres have been converted into jelly, and the columns have fallen asunder, having the appearance and consistence of a translucent very soft mucilage. To what this difference of effect may be owing, I am at a loss to conceive; perhaps the Roman fish were older than the Maltese; or the aqueduct water at Rome may be harder than the rain cistern-water of Malta.

On exposure to the air in a damp atmosphere, or by maceration in water, changing the water daily, the electrical organs undergo change more slowly than the parts distinctly muscular; in putrefaction and maceration they resemble less muscular fibre than tendinous fibre, which latter offers great resistance to both these processes. But I would not lay any stress on this quality of resistance, as it is extremely vague, depending on circumstances which it is difficult to appreciate, as every one must be convinced, who has compared the different degrees of rapidity with which different orders of muscles of man, and the larger mammalia, undergo change from putrefaction and maceration ; for instance, the slowness with which the muscular fibres of the stomach and intestines alter, and the rapidity of change of the fibres of the heart and thick muscles.

Quitting the organs of the dead fish, I shall now notice the few observations which I have made on them before they have been deprived of their vitality.

The effect of the electricity of a small voltaic trough, the shock of which I could just perceive at the extremities of my moistened fingers, was very distinct on the voluntary muscles of a live torpedo, just taken from the water; but it did not appear to affect in the least the electrical organs. I could not perceive the slightest contraction of them in whatever manner the wires were applied, not even when a minute portion of integument was removed, or when one of the wires was placed in contact with a fasciculus of the electrical nerves. Even after apparent death, many of the parts decidedly muscular continued to contract under this stimulus, especially the muscles of the flank, and the cross muscles of the inferior surface of the thorax and the heart; indeed this latter organ, two hours after it had been

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[1] The torpedo is used for food, in the Mediterranean, amongst the lower classes; but the electrical organs are rejected—they are considered unwholesome.

removed from the body, and had ceased to contract spontaneously, renewed its contractions under the galvanic influence.

Other stimulants have been applied to the electrical organs, and with the same negative result. Even when punctured, or incised (a portion of their skin having been removed, which appears to be very sensitive,) no indications whatever were witnessed of their substance being either sensitive or contractile.

Reflecting on the facts and observations which I have just detailed, it appears to me very difficult to resist the conclusion, that the electrical organs of the torpedo are not muscular, but columns formed of tendinous or nervous fibres, distended by thin gelatinous fluid. Their situation, too, surrounded by, and exposed to the pressure of powerful muscles, shows, that if condensation is required for the exercise of the electrical function, they may experience it without possessing any muscular fibres in their own substance. The arrangement of the muscles of the back and of the fins, and of the very powerful cross-muscles, situated between the under surfaces of the electrical organs, are admirably adapted to compress them. Without entering into any minute anatomical examination of these muscles and their uses, it is only necessary to compare them in the torpedo, and in any other species of Ray, to be convinced that they are adequate to, and probably designed for the effect mentioned.

Mr. Hunter,[1] in his account of the torpedo, describes the columns of the electrical organs as composed of cells containing a fluid, divided by horizontal partitions which he was able to count. This structure seems very probable, and in the specimens I dissected at Rome, I saw what I fancied an approach to it; but I have never witnessed it in a satisfactory manner in the fresh fish. Mr. Hunter inspected large fishes which had been preserved in spirits. The partitions of the columns in them might have been more visible (supposing them to exist) from the action of the spirit on the membrane, and from the greater size of the specimen: or they might have been formed after death in the spirits, by a slow deposition of the animal matter contained in the columns.

Next to the nature of the substance of the electrical organs, the electrical nerves have occupied my attention. Their three great trunks have been accurately described by Mr. Hunter; but this distinguished anatomist has very briefly noticed their distribution, which is curious, and deserving, I believe, of minuter investigation. I shall attempt little more than an outline of what I have observed in some dissections conducted with considerable care.

In examining the brain, proceeding from the anterior to the posterior portion, after passing the first, second, third, and fourth pair of nerves, or the olfactory, optic, motor, and pathetic[2] nerves of the

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[1] Philosophical Transactions, 1773.

[2] The nerves of the fourth pair are so very small and tender, that it is difficult to demonstrate them excepting in old and large torpedos.

eye, the fifth pair is seen issuing from the medulla oblongata, or
posterior tubercle of the brain.   After quitting the cranium (con-
fining the description to one side) it proceeds upwards, divides into
two large branches, which go to clusters of mucous glands situated
in the front of the head, and at the interior margin of the electrical
organs, and they appear to be confined to these parts.   The next
pair, the first electrical, rises close to the preceding, just behind it,
and in passing out of the cranium is firmly connected with it; and
also where it passes out, a portion of medullary matter proceeds
from it into a cavity filled with fluid in the cartilage adjoining,
which there is reason to consider as the cavity of the organ of hear-
ing, and the medullary matter the nerve of hearing.   After this, in
passing outwards, it divides into three small branches and two large
ones.   Of the former, one proceeds to the gills, another to the ad-
joining muscles, and the third to the mouth.   Of the great branches,
one ascends, and sweeping round the margin of the electrical organ,
is distributed to the mucous glands which abound there, and where
some of its twigs inosculate with twigs of the former nerve.   The
other great branch, which is inferior, enters the electrical organ,
and ramifies through its superior portion.   The next pair of nerves,
the second electrical, rises a little beyond the preceding.   On leav-
ing the cranium, it divides into two great branches; these, with the
exception of nervous twigs supplying the adjoining branchiæ, are
distributed entirely in the substance of the electrical organ, and
ramify in all directions through its middle portion.   The third elec-
trical rises close to the last, divided only by a very thin plate of car-
tilage.   The principal portion of it passes into the electrical organ,
and ramifies through its inferior part.   Besides, it gives off three
small branches, which are sent to the adjoining branchiæ, to the
gullet and stomach, and to the tail.   The branch which supplies
the stomach appears to be the principal nerve of this organ; it de-
scends along the inner and inferior portion of the gullet, and rami-
fies in the direction of the great arch of the stomach.   The caudal
branch (nervus lateralis) descends in a straight line under the peri-
toneal lining of the abdomen, and under the spinal nerves, without
giving off a single branch, till it reaches the tail, in the muscular
substance of which it is lost.

I have not yet been able to discover any connections of the elec-
trical nerves besides those pointed out.   It is an interesting fact
that the gastric nerves are derived from them.   Perhaps superfluous
electricity, when not required for the defence of the animal, may be
directed to this organ to promote digestion.   In the instance of a
fish which I had in my possession alive many days, and which was
frequently excited to give shocks, digestion appeared to have been
completely arrested; when it died, a small fish was found in its sto-
mach, much in the same state as when it was swallowed: no por-
tion of it had been dissolved.

Though I have not found the temperature of the electrical organs
higher than that of other parts of the fish, or the temperature of the

fish generally different from that of the water in which it has been confined, yet it seems probable that as the branchiæ are liberally supplied with twigs of the electrical nerves, there may be some connection between its respiratory and electrical function; and I venture to offer the conjecture that, by means of its electricity, it may have the power of decomposing water, and of supplying itself with air, when lying covered with mud or sand in situations in which it is easy to conceive pure air may be deficient; and in my experiments I have often fancied that I have witnessed something of the kind; after repeated discharges of its electricity, the margin of the pectoral fins has acquired an appearance as if very minute bubbles of air were generated in it and confined.

Besides the electrical nerves, there is a fasciculus of nerves deserving attention, of great magnitude, formed by the junction of the anterior and posterior, or upper and under cervical nerves; of the former, about seventeen on each side; of the latter, about fourteen.[1] It makes its appearance as one trunk just below the transverse cartilage which is interposed between the thorax and the abdomen. It sends a recurrent branch to the muscles and skin of the under surface of the thorax, but its main trunk ascends along the inner margin of the pectoral fin, and is distributed through it. On this fasciculus the sentient and motive powers of the parts connected with the electrical organs seem to depend.

The electrical nerves at their origin are enveloped in a very thick fibrous sheath. As the branches subdivide in the substance of the organ, the neurilema becomes very thin and semi-transparent. On examining a minute branch with a powerful lens, its internal or medullary substance is not seen in a continuous line, but interrupted, as it were dotted, as if the sheath contained a succession of portions with a little space between each.[2]

In the anatomical structure of the torpedo, the mucous system forms a very conspicuous part; it consists of several clusters and chains of glands, distributed chiefly round the electrical organs, at different depths beneath the cutis, and of strong transparent vessels of various lengths and sizes opening externally on the skin, for the purpose of pouring out the thick mucus secreted by the glands, and destined for lubricating the surface. This system has not been noticed by Mr. Hunter, and it has been but imperfectly described

[1] Towards the origin of the spinal cord, there is a small space, from the under surface of which six nerves arise, three on each side; but none from the upper surface—whence the difference of number noticed in the text.

[2] An after examination, using Ross's microscope, and his object-glass of one eighth of an inch focal distance, has not confirmed the above. Comparing a fibril of an electrical nerve and of a spinal nerve, taken from a fish which had been preserved in spirits four years, there appeared to be no material difference in their minute structure: each appeared irregularly fibrous— that is, composed of fibres not perfectly continuous or exactly parallel, connected by a delicate net-like tissue.

by Lorenzini.[1] Though it is not peculiar to the torpedo, it is much
more strongly developed in this fish than in any other species of
Ray with which I am acquainted; and the situation of the glands
and the distribution of their vessels are different.   Whether it is
concerned in any way with the electrical function of the torpedo,
is deserving of consideration.   That it is thus concerned in some
way, seems to be indicated not only by the situation of these glands
between and surrounding the electrical organs, but still more so by
the manner in which they are supplied with nerves, either from the
first electrical, or from the fourth pair, which is connected with that
nerve.   As the thick semi-transparent mucus which the glands se-
crete is probably a better conductor of electricity than the skin alone
or than salt water, this mucous system may serve as a medium of
communication, not for the opposite surfaces of the electrical organs,
but for each surface individually, included within the line of the
system referred to.[2] I shall mention some results which are favour-
able to this idea.

When one contact wire was placed underneath an active torpedo,
just anterior to the mouth, and the other at the extremity of the
back, out of the circle of the mucous apparatus, the shock of the
fish had no effect either on the multiplier or on the needles in the
spiral.   But when the upper contact wire was made to touch the
back of one electrical organ, the under wire being placed as in the
preceding experiment, then both effects were simultaneously pro-
duced; and they were also produced when the two wires were
brought very close to each other, one being kept as before, and the
other moved immediately over it, in front, each about a quarter of
an inch from the margin, and not connected with the electrical or-
gans, except by the common integuments and this mucous appara-
tus.   It is worthy of remark, that this little space in front, inter-
mediate between the two electrical organs, so abounding in glandu-
lar structure, and so amply provided with nerves, appears from
experiments to possess very little sensibility.   This was denoted in
these trials, in which the fish, though exquisitely sensible of pres-
sure on the margin of the pectoral fins, seemed indifferent to it
when applied in front, as if the fourth pair, which supplies this
part, were destined rather for secretion than for the purpose of sen-
sation.

The connection between the electrical nerves and the mucous

[1] Osservazioni intoruo alle Torpedini fatte de Steffano Lorenzini Fioren-
tino. 4to. Firenze, 1678. Farther on, a more particular account will be
given of the structure.

[2] Some comparative experiments which I have made seem to indicate,
that the mucus of the torpedo is a better conductor than sea-water; when
the hands were smeared with this mucus, or when a portion of the fresh skin
of a torpedo, with its natural mucus adhering to it, was wrapped round the
ends of the contact wires, by which they were held, the shock received ap-
peared to be stronger than usual.

system, even more remarkable than between the former and the stomach, may perhaps warrant the conjecture that the electrical function may not only be aided by, but also aid the secretion of mucus; and that, as was supposed in regard to the stomach, when the electricity is not employed, in repelling an enemy, in violent efforts, it may be exercised gently in increasing the activity of these glands. In support of this notion it may be mentioned, that in the fishes which I have kept, in which digestion was arrested, the se- cretion also of mucus appeared to be stopped, or considerably dimi- nished.

Mr. Hunter, from the examination of a torpedo whose vascular system was injected, states, that the electrical organs of this fish are abundantly supplied with blood-vessels. From what I have wit- nessed in the living fish, and the fresh fish recently dead, I am com- pelled to conclude that the quantity of blood which circulates through them is very inconsiderable. The blood-vessels which pass into them with the electrical nerves are small; the organs are colourless, and very few branches carrying red blood are percepti- ble extending through them. The integuments of these organs, and the pectoral fins, and lateral clusters of mucous glands, are in- deed abundantly supplied with blood-vessels; the contrast of the vascularity of these parts, and of the electrical organs, is so strongly marked as to suggest the idea that the latter can possess very little ordinary vital activity, and that in accordance with the common analogies of living parts, they must be rather passive than active.

Some circumstances pertaining to the integuments of the electri- cal organs are deserving of notice. I have found that the skin covering these organs above is not only more coloured, but also thicker than that covering them below, and more vascular, and sur- rounded by more powerful muscles, and supplied with a greater quantity of mucus; whilst the under surface appears to have a larger proportion of subcutaneous nerves. This difference of struc- ture in the two surfaces of the electrical organs is probably some- how connected with their opposite electrical states.

I may here notice another peculiarity of organisation in the tor- pedo, which came under my observation in seeking, though unsuc- cessfully, for the great sympathetic, or the analogous ganglionic nerves, which Cuvier asserts to exist in the cartilaginous fishes.[1] It has very much the appearance of a nervous ganglion, but is in reality a blood-vessel, enlarged into a little bulb, lined with a red- dish substance, like muscular fibre, giving the idea of a small heart. It is situated one on each side of the aorta, from whence it proceeds, just below the great nervous fasciculus which supplies the pectoral fin, and the arterial branch derived from it is lost in this fin. If it be muscular, as its appearance denotes, its functions may be to aid in propelling the blood into the pectoral fin, and perhaps into the electrical organ.

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[1] Hist. Nat. des Poissons, tom. i. p. 438.

3. — *Theoretical remarks.*

The experiments which I have detailed on the electricity of the torpedo, confirm those of Mr. Walsh, made in 1772, showing its resemblance to common electricity. They moreover show that, like common electricity, and voltaic electricity, it has the power of giving magnetic polarity to iron, and of producing certain chemical changes. In these its general effects, it does not seem to be essentially peculiar, but as much allied to voltaic electricity as voltaic electricity is to atmospheric, or atmospheric electricity is to that produced by contact or friction. When we examine more minutely its phenomena or effects, in relation to these different kinds or varieties of electricity, certain points of difference occur.

Compared with voltaic electricity, its effect on the multiplier is feeble; its power of decomposing water and metallic solutions is inconsiderable; but its power of giving a shock is great, and so also is its power of magnetising iron.

Compared with common electricity, it has a power of affecting the multiplier, which, under ordinary circumstances, common electricity does not exhibit; its chemical effects are more distinct; its power of magnetising iron[1] and giving a shock appear very similar; its power of passing through air is infinitely less; as is also its power of producing heat.

There are other points of difference; I allude chiefly to the results obtained in the experiments already described, in which the metallic communication was interrupted by a strong solution of salt. In this instance the full power of the fish appeared to pass; water was decomposed, a shock was received, needles were magnetised, and the multiplier was affected. When the same experiment was made on the electricity excited by the small voltaic combination of a single plate of copper and zinc, each less than an inch in length and half an inch in breadth, immersed in an acid, neither water was decomposed, nor was the multiplier affected. When it was made on the electricity of the electrical machine by means of a Leyden jar, all the effects were witnessed, excepting the motion of the multiplier, and the order of succession of poles in the needles magnetised in the spirals. How are these differences to be explained? Do they admit of explanation similar to that advanced by Mr. Cavendish in his theory of the torpedo, and which has recently been ably advocated by Mr. Faraday? or are they more in accordance with the idea which my brother was disposed to adopt, that the electricity of the torpedo and of the other electrical fishes

[1] There is this difference when two spirals are used, one connected with the inside of a Leyden jar, and the other with the outside; a needle in each, similarly placed, acquires opposite polarities, the north pole in one being where the south pole is in the other; whilst in the instance of the torpedo they accord, so that a line of needles, passing from one end of the electrical organ to the other, would exhibit a succession of similar poles.

is peculiar—a power *sui generis?* or, lastly, may we suppose, ac-
cording to the analogy of the solar ray, that it is not a single power,
but a combination of powers?

The first opinion, which is commonly received, is supported by
the majority of facts detailed in the preceding pages. The circum-
stance principally hostile to it, at least in appearance, is the inter-
ruption of the torpedinal electricity by the smallest quantity of air,
and its want of the power of attraction and repulsion in the air.

These peculiarities are seemingly in favour of the second opinion,
that the electricity of the torpedo is specific and peculiar. But till
the opposite surfaces of the electrical organs can be perfectly insu-
lated, so that no easier mode of communication is afforded than
through the air, they can hardly be considered of much weight.[1]

The third opinion may be indulged in as an hypothesis; as a
guide to research it may not be useless. It applies, however, almost
as much to other varieties of electricity as to that of the torpedo;
all of which, it is possible, may be compounded, or owe their va-
rious effects to the union of several powers, or etherial fluids, and
their peculiarities, compared one with another, to the predominance
in various degrees of these fluids. What is known of the solar ray
is not unfavourable to such an opinion; and the history of physi-
cal science, in relation to elementary ponderable matter, may give
rather encouragement to the notion.

As regards the mode of production, or the cause of the electricity
of the torpedo, it is unavoidably enveloped in great mystery. Like
animal heat, and the light emitted by certain animals, and I may
add, the secretions of animals generally, it appears to be the result
of living action; and connected with a peculiar and unusually com-
plicated organisation. All the attempts have been vain, which I
have made, directed to obtain electrical excitement in the fish after
it had been deprived of life.

The observations which I have detailed relating to its anatomical
structure, show a complicated adaptation of parts—nerves of un-
usual magnitude ramifying between apparently insensible columns,
saturated with a bad conducting fluid; muscles surrounding these
columns, and fitted to compress them; and a system of mucous
glands and tubes adjoining, well adapted to be the medium of elec-
trical communication in the surfaces to which they belong.

When we consider this structure, it is an easy matter to trace
rude analogies between it and the pile of Volta, or between its co-
lumns and a battery of Leyden jars, such a battery as was formed
by Mr. Cavendish for imitating the electricity of the torpedo, com-
posed of a large number of jars of very thin glass feebly charged.
But these analogies seem to help very little, if at all, towards the

[1] In the experiments in which I attempted to insulate the surfaces, by
means of oil, the probability is that I failed, and that a communication con-
tinued, if not by the outer surface of the skin, at least by its inner; indeed,
the attempts to insulate these organs, in the manner desired, seems to be
almost hopeless.

11—f   17 *dav*

solution of the great difficulty; the question remains unanswered, what is the cause or source of the electricity? Here analogy fails entirely; none of the ordinary modes of excitement appear to be at all concerned—neither friction, nor chemical action, or change of temperature, or change of form.   Let us consider for a moment a small torpedo in an active state.   The smallest which I have employed in my experiments weighed only 410 grains, and contained only 48 grains of solid matter; its electrical organs weighed only 150 grains, and contained only 14 grains of solid matter—for to this they were reduced by thorough drying.   Yet this small mass of matter gave sharp shocks, converted needles into magnets, affected distinctly the multiplier, and acted as a chemical agent, effecting the decomposition of water, &c.   *A priori*, how inconceivable that these effects could be so produced !   This fish was about ten days in my possession, during the whole of which time it ate nothing, and its bulk was hardly sensibly altered; and every day it exercised its electrical powers, and to the last they appeared almost as energetic as when it was fresh from the sea.   This adds, if possible, to the difficulty of explanation.   That this mysterious function is intimately connected with the nerves, and in a manner more striking than all ordinary secretions, is manifest.   Beyond this conclusion, all is darkness.   We have not, as we have in the doctrine of animal heat, advanced another step—we have not been able to connect it with changes in the electrical organs, as analogous to known sources of electricity, as the changes which take place in the lungs in respiration are to the known sources of heat or combustion. The attainment of this step is a great desideratum, and beyond it, probably, we shall never be able to proceed.

Without reverting to the conjectures, which in passing I have offered on the subserviency of the electricity of the torpedo in an auxiliary manner to digestion, respiration, and the secretion of mucus, I may remark that its chief use appears to be for purposes of defence—to guard it from its enemies, rather than to enable it, according to vulgar opinion, to destroy its prey, and provide itself with food.   Small smelts, which were kept in the same vessel with torpedos, appeared to have no dread of them, and I believe they fed on their mucus.   And, in an experiment in which, in a confined space, an active torpedo was excited to give shocks, a smelt which was with it was evidently alarmed, and once or twice exposed to the shock, leapt nearly out of the vessel; but was not injured by the electricity.   In confirmation, I may add that the electric power of the young fish, which most requires it for its protection, is, as already observed, proportionally very much greater than that of the old, and can be excited without exhaustion and loss of life, much more frequently.   After a very few shocks, most of the old fish which I had have become languid, and have died in a few hours; whilst young ones from three to six inches long have remained active during ten or fifteen days, and have never failed to show the effects I have described.

4.— *Of the other electrical animals compared with the Torpedo.*

The number of animals of this description hitherto discovered and well authenticated is very small; in brief, the Gymnotus Electricus and the Silurus Electricus, besides the torpedo, are the only ones, the existence of which is certain and free from all doubt;[1] and to these I shall confine the few remarks which I propose to offer relative to their comparative powers and structure.

All these fishes have certain points of resemblance. Independent of their electrical powers, they are peculiarly defenceless; their skin is soft—a true mucous membrane, although external, totally destitute, equally of scales and of spines. Each possesses a supplementary organ with which their electrical function is connected; and in each the nerves supplying these organs are of extraordinary magnitude.

The electrical organs of the respective fishes bear to each other in their structure a general, but not close, resemblance. The organs of the torpedo are distinctly columnar—the columns very numerous; those of the gymnotus consist of septa, or of flat partitions with cross divisions between them: whilst those of the silurus are neither distinctly columnar, nor cellular, being formed as it were of a delicate net-work of tendinous fibres, retaining interposed a soft gelatinous matter, somewhat similar to that contained in the fibrous columnar or cellular structure of the organs of the former fishes.

The nerves supplying the organs are peculiarly deserving of remark. It is a curious circumstance, that in each instance their source is different.

In the torpedo, we have seen that they are derived exclusively from the brain, from its posterior tubercle.

[1] In the list of electrical fishes, two others are commonly introduced—the so named Tetraodon Electricus and Trichiurus Electricus; but neither on good authority. The information on which the existence of the first has been received, is most scanty and imperfect. The narrative has a good deal the manner of fiction. It is contained in the Philosophical Transactions for 1786, in a letter from Lieut. William Paterson, of the 98th Regiment, to Sir Joseph Banks, accompanied with a drawing. The writer states, that when accompanying his regiment to India, in the Island of Johanna, where they touched, he met with a new species of electrical fish, of the genus Tetraodon, as he supposes, in cavities on the shore in the coral rock, and that in handling one he received a shock. He says, he saw several of them, and that the water in which they were was about 55° or 60° of Fahrenheit, although the island is within the tropics (sea-water of 55° or 60° in lat. 12° 13′ south!)

The reality of the other fish, Trichiurus Electricus, is more than doubtful, although so called by Lacépède and by Shaw. The author of the " Supplement on the first order of Fish," to the English translation of Cuvier's " Regne Animal," remarks, " that the characters and properties announced with so much assurance, rest only on a bad figure given by Nieuhoff, and a transposition of a part of the text, which has no reference to the figure, nor to any trichiurus." Vol. x. p. 348. How true is the remark, in natural as well as in civil history, that " men love to complete what is imperfect, and to realise what is imaginary."

In the gymnotus electricus, they do not appear to come at all from the brain, but entirely from the spinal cord. This was the result of Hunter's dissection; he says, " the organ is supplied with nerves from the medulla spinalis, from which they come out in pairs between all the vertebræ of the spine."

In the silurus electricus, according to the researches of Rudelphi, they appear to have a double source; those supplying the outer organ being derived from the fifth pair; those supplying the inner, from the intercostal.

Moreover, a marked difference appears in the proportional magnitude of the nerves in the several instances. In the torpedo they appear to be largest; in the silurus smallest. Hunter says, "if all the nerves which go to it (the electrical organ of the torpedo) were united together, they would make a vastly greater chord than all those which go to the organs of this eel," (the electrical gymnotus.)[1] Their comparative smallness in the silurus, I have satisfied myself of by my own observations, made on a specimen, which through the kindness of the American consul at Malta, I procured from Egypt, and which is now deposited in the museum at Fort Pitt.

As regards electrical power, it is commonly believed, that it does not accord with the proportion of nerve; that it is feebler in the torpedo than in the silurus, and greatest in the gymnotus.

In relation to the shock or sensation which these fishes impart, the opinion probably is true. What I have heard in conversation from that distinguished traveller and naturalist, Dr. Rouppell, would seem to indicate that the shock of the silurus is more energetic than that of the torpedo; and that that of the gymnotus is the most energetic, seems not to admit of doubt, considering the well authenticated accounts we have of its effects on large animals, such as the horse, especially as related by M. De Humboldt.

But it does not, therefore, necessarily follow, that the power is in other respects proportional; for instance, as a chemical, heating, or a magnetising agent. Certain forms of electricity differ in these manifestations, and why not the electrical power of these fishes?

The question can only be determined by actual experiments. The arrival of a gymnotus in England, with liberal permission from the proprietors of the Adelaide Gallery of Practical Science to make trial of it, has enabled me to do this in a partial manner. It was on the 29th of November last, that I made the trial, when I was assured by Mr. Bradley, that the fish was, as it appeared to be, in excellent health. For the sake of comparison, I used the same apparatus as I had employed at Malta on the torpedo, and in the same manner. The first experiment attempted was on the galvanometer. The effect of the electrical discharge of the fish was very decided; the needle made nearly half a circle. The next experiment was on two small needles in a spiral connected with the galvanometer. The galvanometer was powerfully affected, as in the first instance, but

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[1] Philosophical Transactions for 1775.

the needles were not sensibly magnetised ; the trial was twice or thrice made with the same results.   Next, a shock was received. Mr. Owen, to whose kind offices I was in part indebted for the per- mission I had obtained, had prepared me from the mention of his own experience to expect that this would not be contemptible : it rather exceeded than fell short of my expectations : the sensation was rather like that imparted by a Leyden vial than the voltaic battery ; more acute, less benumbing than that of the torpedo ; and it reached higher, even beyond the elbows.[1]   I did not extend the trials further, nor attempt to test it as a chemical agent, having been informed that Mr. Faraday, who preceded me in experimenting on this fish, had carefully investigated its influence in relation to this point.

Since that time this gentleman has communicated the results which he obtained to the Royal Society, from the proceedings of which it would appear that, besides the spark, he has witnessed all the effects known to have been produced by the electricity of the torpedo.

Reasoning from my own comparative results on the electricity of the torpedo and gymnotus, it appears to me difficult to avoid the conclusion, that the power of each, whatever it may be in essence, is modified in its manifestations, or effects ; the difference between the electricities of the two fishes in imparting shocks, and in mag- netising steel, was too strongly marked to be in any wise doubtful.[2]

XX.

ON THE URINARY ORGANS AND SECRETIONS OF SOME OF THE AMPHIBIA.

During a period of nearly four years that I was stationed in Cey- lon, viz. from 1816 to 1820, I availed myself of the favourable op- portunities which there offered to examine the urinary organs and

[1] At the obliging suggestions of Mr. Bradley, by whose directions every assistance I could wish was afforded me, the insulated wires had attached to them bent pieces of sheet-copper, in the form of a saddle, for better con- tact with the fish.   These were used in the two first experiments, and also in a fourth, when I had the pleasure of witnessing the production of sparks, —according to the method by which M. Matteucci, in 1836, succeeded when experimenting on the torpedo, and Mr. Faraday, recently on this fish ; in the third trial, the hands were applied immediately to the surface of the fish.

[2] [The sections on the fœtal developement of the torpedo, the species of torpedo in the Mediterranean, and on the structure of the torpedo, requiring for their illustration a number of expensive plates, and being withal intended for the naturalist especially, have been omitted.—ED.]

urine of many of the animals comprehended in Linnæus's class of
Amphibia.

Previously, the subject had received but little attention ; the or-
gans had been very imperfectly described, and no account that has
come to my knowledge, had then been published, of the chemical
nature of their secretion.

The results of my observations were communicated to the Royal
Society ; they were honoured with a place in the Philosophical
Transactions ; and were mainly as follows.

1.—*Of the urinary organs and urine of Serpents.*

The kidneys of several different kinds of serpents, which were
submitted to examination, resembled each other generally, although
in each kind there were minute and trifling differences. In every
instance they were very large, nearly equal in size to the liver; they
were long and narrow, and very lobulated ; and like those of some
of the mammalia, with conglomerate kidneys, they were destitute
of a pelvis, each lobule sending a small duct to the ureter.   Each
ureter, formed by the union of two branches, terminated in a single
papilla, situated in the cloaca, and a little elevated above its surface,
the point of the papilla containing its orifice, directed towards a re-
ceptacle, into which the urine entered. The receptacle here alluded
to is a continuation of the intestine, yet it may be considered distinct
both from the rectum and cloaca, with which it communicated only
by means of sphincter orifices. This conformation of parts was seen
to advantage in the larger species of snakes ; I first observed it in
the python, and in a large coluber, commonly called the rat-snake,
frequently met with from eight to ten feet long.

The urinary ducts, even in the substance of the kidneys, were
often observed to be of an opaque white colour, owing to a white
matter which they contained, which was visible through their trans-
parent coats, and which could be expressed from the papilla, and
collected in small quantities for examination.   More or less of a
similar white matter was almost constantly found in the receptacle,
generally in soft lumps, rarely in hard masses.   In the receptacle, I
always observed it pure, and not entirely free from fœcal matter.
This solid urine, for such it was in reality, had gradually accumu-
lated in the receptacle, until it formed the masses just described.
From the observations made on snakes which were watched in con-
finement, it was to be inferred that the masses were a considerable time
in collecting, from three weeks to a month or six weeks. When the
bulk of the masses was so large as to distend the part unduly, they
were expelled by an unusual exertion of the animal, most commonly
in the act of devouring its food, which was usually taken periodi-
cally, at intervals of from three to six weeks.   The urine, in the
instances in which it was observed, was voided, accompanied occa-
sionally but never mixed with fæces.   When expelled, it was com-
monly in a soft state, of a butyraceous consistence, which it lost

from exposure to the air, when it became hard and in appearance like chalk; a change which I believe was owing merely to the evaporation of moisture. The quantity of solid urine secreted by snakes, is very great, even more than might be expected from the size of their kidneys; I often saw masses, voided by large snakes, which weighed three or four ounces.

The chemical nature of this urine was such as I expected to find it: I say expected, because, before I left England, I had been told by Dr. Prout, that he had examined the excrement of a serpent in London, and had ascertained that it was nearly pure lithic acid. Such I found it in every instance, in at least eight in which I tried it; and its properties, whether fresh from the ureter, or after it had collected in the receptacle, or later when voided, were not materially different, excepting, as I believe (and this is matter of after-inference formed on reflection) that the fresh secretion contained no free ammonia, or even lithate of ammonia, the existence of which, I apprehend, when it occurs, is owing to partial decomposition. Subjected to examination before the blow-pipe, it emitted strong ammoniacal fumes, consumed without flame, and afforded only a very minute quantity of ash, which consisted chiefly of phosphate of lime, and a fixed alkaline phosphate, and a little carbonate of lime. In muriatic acid, it was insoluble. In warm dilute nitric acid, it dissolved with effervescence, and the solution evaporated afforded the pink residue, almost peculiar to lithic acid. In an alkaline ley, it was soluble, and the solution was precipitated by muriatic acid. These properties sufficiently proved that the nature of the urine was as above stated. Besides lithic acid, I was not able to detect any other ingredient, nor do I believe that the specimens which I examined contained any other with the exception of a minute quantity of mucus with which they were lubricated and mixed.

2.— Of the urinary organs and urine of Lizards.

Whilst in Ceylon, I examined the urinary organs of four different species of lizard; the gecko, iguana, a large species resembling the iguana, called by the natives kobbera-guion, and the alligator. The shape of the kidneys was found to vary in the different species; in their general structure they appeared to be essentially the same. Each ureter terminated in a distinct papilla, and both papillæ were situated in the receptacle itself. In no other respect was I able to discover between the urinary organs of these lizards, and the snakes which I dissected, any material difference. Neither did the urinary secretion of these four species, and of many other species which I then and have since analysed, differ from that of snakes in its essential nature; in every instance it proved to be nearly pure lithic acid. Two specimens of urine, from different alligators, agreed in this circumstance; they differed, however, in one having no odour, the other a strong odour of musk; the former was from a very young animal, the other was from an older one.

3.—*Of the urinary organs and urine of the Turtle and Tortoise.*

The only species of Testudo, the urinary organs of which I have examined, are the mydas, geometrica, and græca ; the two former in Ceylon ; the last, recently in this country.   Their kidneys, in their lobulated structure, resemble those of the preceding animals.   The proportional size of the kidneys of snakes is greatest, that of the lizards next, and that of the turtle and tortoise least.   As is well known, these latter animals differ from the former, in having a urinary bladder.   The large green turtle of Ceylon has a pear-shaped urinary bladder, not unlike that of some of the mammalia, communicating with the cloaca by a long dilatable neck, or, as it may be considered, urethra, in which, nearer its termination in the intestine than its origin, the ureters open by two large prominent papillæ.   The Testudo græca has a large bladder, much resembling that of the toad, of the like delicate structure, and divided into two compartments posteriorly ; but similar to the turtle, in its communication with the cloaca and the termination of the ureters, with this slight difference, however, that they are still more distant from the bladder, opening close to the extremity of the urethra.

In the bladder, both of the turtle and tortoise of Ceylon, I found flakes of pure lithic acid ; but in no great abundance : they were suspended in a transparent watery fluid, containing a little mucus and common salt, but no urea, or any other substance, which I could detect in the small quantity the subject of experiment.

The urinary bladder of the Testudo græca was in a state of distention. Its contents were found to consist of lithic acid and lithate of ammonia, partly in little firm masses, and partly in the form of sand and gravel, not crystalline, mixed with a little urea.   The latter was in solution ; it was detected in a very minute quantity of brown liquid, the only portion of the urine that was fluid.

This tortoise, it may be remarked, was brought from the Mediterranean (I believe, from the Ionian Islands) in the summer of 1836. It passed the winter, according to its habit, in a torpid state. It was on the first of March following, that it was subjected to examination. Then its period of hybernation was just over ; it had begun to show signs of activity.   For several months it had ate nothing, and, it is believed, had not voided excrement of any kind.   The temperature of the air of the room at the time was 55°, out of doors 50°, the thermometer placed in the rectum, fell to 48°, in the blood flowing from the right auricle, it rose to 50°, and in that from the ventricle to $51^\circ.5$.

4.—*Of the urinary organs and urine of the Frog and Toad.*

The examination which I made of the urinary organs of these animals in Ceylon, was confined to one species of each,—the bullfrog (Rana taurina, Cuv.) and the brown-toad (Bufo fuscus, Lauren-

tini.)   The former was procured from the lake of Colombo, where it occasionally grows to a great size;—the latter from the streets of the Pettah, which it frequents at night.

The kidneys of the bull-frog, on examination, were found to be apart, one on each side of the spine;—comparatively pretty large,— very much lobulated,—of a bright red colour, and rather tender. The ureters did not terminate in the bladder but in the rectum, by two soft papillæ, projecting a little, and situated between the orifice of the bladder and the anus,—nearer the former than the latter. The urinary bladder was of large dimensions, nearly globular, semi-transparent, and yet pretty strong and contractile, and evidently muscular.   It opened into the rectum a few lines behind the anus, by a large orifice very well adapted to receive the urine, as it flows from the ureters, when the anus is closed, as it usually is during the life of the animal and soon after death, by its powerful sphincter muscle.

The urinary organs of the brown toad resembled in most respects those of the green frog.   In two specimens out of many that I dissected, I found the kidneys incorporated at their upper ends. The ureters had the same termination nearly.   The bladder of urine appeared to be double; when distended fully with air, it resembled two oval bags; which, as two compartments, communicated freely just above the symphisis pubis, to which they were firmly attached; and they communicated also by a single orifice with the rectum.   This orifice was as well suited as that of the former for the reception of the urine as it flows into the rectum.   The urine of the bull-frog, taken from the bladder immediately after the death of the animal, varied a little in its appearance in different instances, and very considerably in quantity, the bladder being sometimes full almost to distention, and at other times quite empty.   The following is a description of a quantity of urine, which amounted to 300 grains, and which was collected from thirty-six frogs of different sizes.

It was almost transparent and like water in appearance   It was insipid, but was not without smell; the odour from it was not unlike that of the serum of blood.   It was of specific gravity 1003.   It had no effect on litmus or turmeric paper.   Slowly evaporated, it afforded a minute quantity of brownish extract which had the smell of urea before it was purified.   This deliquesced on exposure to the air. Decomposed by heat in a small glass tube, it yielded a little amber-coloured fluid and strong ammoniacal fumes, and a residual coal, in which was detected a large proportion of common salt and a little phosphate of lime.

Another specimen of the urine of these frogs was found to be more dilute; it contained a minute portion of common salt and of phosphate of lime, without any appreciable quantity of urea.

The urine of the brown toad was pretty uniform in its appearance in different instances; and in its composition also, judging from the results of the experiments made on it.   From the bladders of eighty-four toads, 732 grains of urine were collected.   Examined

when quite fresh, it was nearly transparent, and would have been perfectly so, but for a few minute flocculi suspended in it. It was of a pretty bright straw-yellow, very like healthy human urine in appearance, with the peculiar smell, and nearly the same taste in a slight degree. It was of specific gravity 1008. It did not alter litmus or turmeric paper. Nitrate of silver dropped into it produced a very copious precipitate of luna cornea. A solution of corrosive sublimate occasioned a minute flocculent precipitate; neutral acetate of lead, a copious white precipitate. *Aqua ammoniæ* had no effect; oxalate of ammonia produced a slight cloudiness; and a faint cloudiness was produced by muriate of barytes, which did not disappear on the addition of a drop of nitric acid. A portion of this urine slowly evaporated, afforded a brown extract, with a strong urinous smell. To the moiety of this extract of a syrupy consistence, a drop of nitric acid was added; the effect produced was just the same as if concentrated human urine had been the subject of the experiment; a crystalline compound immediately formed, which I could not hesitate in pronouncing nitrate of urea. The other moiety, decomposed by heat in a glass tube closed at one end, afforded a considerable quantity of yellow oily fluid, strongly impregnated with carbonate of ammonia, and a residual coal, from which was obtained a large proportion of common salt, and a little phosphate of lime, and slight traces of a fixed alkaline phosphate. Another portion of this urine was set aside to undergo spontaneous decomposition. After having been kept eight days, it became slightly turbid and emitted a distinct, though not strong, ammoniacal odour.

The conclusions to be drawn from the results of these experiments hardly require to be pointed out, viz. that the urine of the bull-frog and of the brown toad contained urea as a constituent part, and the latter rather abundantly. And reasoning from analogy, it appears highly probable, that the urine of frogs and toads in general, is of a similar nature, and not altogether different from that of the other amphibia.

It is seldom that any very abrupt transitions are to be observed in nature: the urinary organs of the turtle and tortoise, on the one hand, seem to be a connecting link between those of the reptiles and the mammalia, especially as regards their form of bladder, and the termination of their ureters; and on the other, amongst the reptiles themselves, to form a gradation between the snakes and the batrachians.[1]

Perhaps additional facts are not required to prove that the nature of the secretion of the kidneys of animals depends more on the intimate and invisible structure of these organs, than on the kind of food the animals consume. Were such facts wanting, there would be no difficulty in furnishing them. How different is the urine of the brown toad and that of any species of small lizard! Yet flies are the fa-

[1] The gradation is even more complete through the *Menobranchus lateralis* of the Canadian lakes: its bladder is very small: its ureters terminate in the cloaca, close to the entrance of the bladder.

vomite and common diet of both.   Other remarkable instances
might be mentioned of similarity of diet, and difference of urinary
secretion ; and *vice versa*, instances might be afforded of difference
of diet and similarity of urine : I will mention one only ; it is that of
parrots and snakes ; their urine, as I have found, being much the
same, consisting chiefly of lithic acid, though their diet is altogether
different, the birds feeding entirely on vegetable matter,—the rep-
tiles entirely on animal matter.[1]   But let me not be supposed to
maintain that the urinary secretion depends entirely on the organ,
quite independent of the nature of the food, or of the blood, from
which the elements of the urine are derived.   It appears to be pretty
satisfactorily proved, that, *cæteris paribus*, there is a certain relation
between the nature of the food and of the urine.   Whilst this has
been generally admitted, the relation between the organ and the se-
cretion has been less insisted on, though perhaps not less curious
and deserving of attention.

XXI.

ON THE ACRID FLUID SECRETED BY THE COMMON TOAD.

In every country in which the toad is found, it is considered
poisonous by the common people ; and the opinion may be traced
back to a very remote antiquity.   Of late years the notion has been
rejected by the professed naturalists, and placed in the number of
vulgar prejudices.   Thus M. Cuvier, in his general remarks on the
species, says " Ce sont des animaux d'une forme hideuse, dégoûtant,
que l'on accuse mal-à-propos d'être venimeux par leur salive, leur
morsure, leur urine, et même par l'humeur qu'ils transpirent."[2]

From observations which I made to endeavour to determine this
point in the Ionian Islands, in 1825, I was obliged to come to the
conclusion, that the popular opinion was not without foundation,
and, of the two, the most correct.   The results of my experiments on
the subject, were communicated to the Royal Society, in the year in
which they were made, and they were published in the Philosophical
Transactions for 1826 ; they were limited to the common toad,
(Rana Bufo, L.) and were nearly as follows :

In carefully examining the toad, I found it possessed of a pecu-
liar fluid, seated chiefly in the integuments, in follicles, in the cutis

[1] The land tortoise of the shores of the Mediterranean is another remark-
able instance.   I have mentioned how I found its urine to consist chiefly of
lithic acid ; yet its food is chiefly, indeed I believe, exclusively vegetable
matter.

[2] " Le Règne Animal," tom. ii. p. 94. Paris, 1817.   The remark is repeated
in the last edition, that of 1827.

vera, beneath the cuticle and the coloured rete mucosum.   These follicles were largest and most numerous near the shoulders and about the neck of the animal; but they were far from being confined to these parts, they were very generally distributed, and even on the extremities.   Pressure being applied to the skin, a yellowish thick fluid, the fluid in question, exuded, occasionally it even spurted to a considerable distance.   No difficulty was experienced in collecting it, from large and old animals, in sufficient quantity for examination.   The following are some of the properties of the specimen which was subjected to experiment.

The greater part of it was soluble in alcohol and water.   The aqueous solution was slightly viscid, and did not pass readily through a common filter.   It was not precipitated by acetate of lead; and its transparency was very slightly impaired by a solution of corrosive sublimate.   The substance obtained by evaporation, both from the aqueous and alcoholic solution, was light yellow and transparent; had a faint and peculiar smell different from that of the toad; and it was slightly bitter and very acrid, acting on the tongue like the extract of aconite prepared in vacuo; and even occasioning a smarting sensation when applied to the skin of the hand; its effect lasted two or three hours.   When heated, it readily melted; burnt with a bright flame, and did not emit an ammoniacal odour.   It did not appear to be either acid or alkaline, judging from its not changing the colour of turmeric, or of litmus paper.   Caustic ammonia (*aqua ammoniæ*) dissolved it, without depriving it of its acrid quality.   Nitric acid also dissolved it; forming a purple coloured solution, which, neutralised by an alkali, was less acrid, as if partially decomposed.   The small portion of the fluid, not soluble either in water or alcohol, and to which it owed its consistence, was probably a variety of albumen; and its appearance, when burning, would seem to warrant this idea.

Though this fluid of the toad was more acrid than the poison of the most venomous snakes, it did not appear to have any injurious, and much less fatal, effects, when absorbed and carried into the circulation of an animal on which it was tried: thus, on a chicken, punctured with a lancet which had been dipped in it, received no apparent injury.   And, in confirmation of this statement, I may remark, that although, as already observed, it abounds chiefly in the integuments, it is not confined to them; I found it in notable quantity in the bile, in minute quantity in the viscid fluid lubricating the tongue, and also in the urine, and even in the blood.

In my paper, presented to the Royal Society, I called this fluid a poison; considering the properties described, perhaps it may with more propriety be called an acrid than a poisonous fluid; and rather analogous to that secreted by certain insects, as the bee, and scorpion, than to the more virulent and less acrid fluid of the poisonous snakes.

Reflecting on the use which this fluid may be of to the toad, it

occurred to me, that it may answer two purposes, and these of importance to this abhorred but innocent reptile.

As the external surface of the skin is smeared with this "sweltered venom," as it has been called by our great dramatic poet, it must serve to defend it from the attacks of carnivorous animals : "a toad to eat," is a proverbial expression well known ; and the facts adduced show its propriety and force. I may here add, that nature has given this sluggish and helpless animal an additional security against attack, in providing it with integuments of great thickness, and strength, and hardness; which last-mentioned quality is imparted by a layer, between the fibrous cutis, (the cutis vera) and rete mucosum, almost analogous to bone, abounding in phosphate and carbonate of lime, and carbonate of magnesia, semi-transparent, and yet so firm, that it is not easily cut.[1]

As the fluid contains a substance which is very inflammable, and as it may be considered excrementitious, though the blood is very slightly impregnated with it, it may serve to separate a portion of carbon from the blood, and thus in its function be auxiliary to the func-

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[1] The tegumentary system of reptiles is curiously varied in structure and composition ; in every instance admirably adapted to the peculiar habits of the species. How great is the contrast between the soft mucous covering of aquatic active frogs, and the firm, hard, envenomed covering of the sluggish land-toads ; or of the former, and the tortoise; or of the different species of lizards, compared one with another, in relation to their skin, in connection with their habits and defensive wants against their enemies. A careful chemical examination of the integuments of reptiles, I have no doubt, would amply repay and bring to light many curious and interesting facts. My own labours in this field have hitherto been very limited and partial. Besides the skin of the toad, I have only examined that of two other reptiles, viz. the python and Indian alligator. The first dry, I found to consist of

> 96.7 animal matter.
> 3.3 earthy matter.

> 100.0

The earthy matter consisted of phosphate of lime and silica, the latter in large proportion, coloured reddish by peroxide of iron.

The second dry (a single plate from the back which weighed 88.2 grs. was the subject of experiment, and it was further dried at a temperature of about 212, previous to examination,) was found to contain

> 5.7 water (hygrometric)
> 33.6 earthy matter
> 60.7 animal matter.

Subjected to a strong heat in an open crucible, the animal part consumed with flame, and afforded a residue retaining the form of the plate, differing from the original only in being somewhat shrunk. This bony plate consisted principally of phosphate of lime, besides which it contained a little carbonate of lime and silica. On the inner surface of the plate there were minute concretions of quartz, the largest about the size of a small pin's head, semi-transparent, but not distinctly crystalline. Their hardness, and not being acted on by an acid, were properties sufficiently characteristic of their nature.

Whether the presence of silica in these instances is to be considered as accidental or the effect of disease, or constant and normal, analogous to what occurs in the protecting epidermis of some plants, remains to be ascertained.

tion of the lungs.   In support of this idea I may remark, that I have
found the pulmonary arteries of the toad, each divided into two
branches ; one of which went to its respective lung, and the other,
very little smaller, to the cutis, between the head and shoulder on
each side, and was extensively ramified where the largest follicles
were situated, and where there was a plexus of veins of great size,
as if intended for a reservoir of blood.

This last-mentioned peculiarity of structure, and the situation of
it, corresponding to the gills of the tad-pole, would seem to indicate
that the subcutaneous distribution of the second branch of the pul-
monary artery, may aid the lungs further in their office by bring-
ing the blood to the surface to be acted on by atmospheric air.

I have endeavoured to ascertain if there is any direct communica-
tion by spiracula through the integuments.   The results obtained
have been negative.   Air has been introduced by means of a forc-
ing syringe, under the loose skin, through a small incision, also into
the cavity of the abdomen, and into the lungs by the superior glot-
tis ; and has been much compressed, in these parts under water, yet
it has been completely confined—not even the smallest bubble was
visible—forced either through the skin or through the lungs.

When dried, the skin of the toad, I find, exhibits two kinds of
pores ; one kind, few in number, confined to the tuberosities over the
shoulders, sufficiently large to receive a hog's bristle ; the other kind,
very numerous, scattered over the whole surface, and very minute.
Both of them were best seen by holding the skin between the eye
and a strong light ; the smallest appeared as luminous points of a
yellow hue, the largest as indistinct circles.   Both were covered
externally with transparent cuticle, and internally by a delicate sur-
face of cellular tissue ; some of the largest were also covered with
rete mucosum ; the smallest appeared to be destitute of this mem-
brane : both, I believe, are destitute of the superficial layer or crust,
already mentioned, consisting chiefly of phosphate of lime.

Whether these apparent pores are the medium of communication
between the blood in the subcutaneous capillary vessels and the at-
mosphere, or whether they are merely parts of the cutaneous appa-
ratus of secretion and exudation, it is difficult to determine ; it is not
improbable that they perform both functions : the experiments of
Spallanzani, and the very ingenious ones of Dr. Edwards, on what
has been denominated cutaneous respiration, may be considered as
demonstrative, that they perform at least the first-named function.

XXII.

ON THE POISON OF THREE OF THE POISONOUS SNAKES OF CEYLON.

Of twenty different species of snakes which I had an opportunity of examining in Ceylon, four only belonged to the poisonous kind; namely, the Hooded-snake, the Tic polonga, the carawilla (as they are called by the natives) and the Trigonocephalus nigro-marginatus, (the Bodroo Pam of Russel.[1]) All these, with the exception of the last, of which, from its extreme rareness, I could not procure a living specimen, I tried on animals, with a view to endeavour to ascertain what are the effects of their poison. Owing to an unavoidable interruption, to which medical officers are liable on service, the inquiry was not carried nearly so far as I could have wished : I was obliged to leave it in an unfinished state. However, as the results which I had obtained appeared to me to be of some value (as the results of all experiments carefully made and honestly related, necessarily must be) I thought it right to introduce them into my work on the Interior of Ceylon, which was published in 1821. From that account the following is an extract. I am induced to give it, partly for the reason already assigned, and partly in consideration that the work referred to is scarce, and out of print; and, moreover, as it appears desirable to call attention to a subject of acknowledged interest and importance, obscure and mysterious in itself, and peculiarly requiring various and multiplied elucidation by experiments.

1.—*On the poison of the Hooded snake* (*Naia tripudians, Marrem.*)

Experiment 1.—The snake used in this experiment was about five feet long and about six inches in circumference in its broadest part. It appeared to be active and in good health. On the 30th of November, 1816, at Colombo, a full grown hen was brought near it. After much threatening the snake darted on the hen, and fixed its fangs in the skin, covering the lower part of the pectoral muscle. It kept its hold about two or three seconds, when I succeeded in shaking it off.

At the moment, the hen appeared to be but little affected; she seemed rather uneasy and restless, and was every now and then pecking the part bitten. Some corn being thrown on the ground, she ate only a very little. In two hours she was worse; but even then the action of the poison was not apparent from any remarkable symptom, merely from a certain degree of debility and languor, indicated by her being easily caught, and by crouching when not disturbed. There was no swelling or appearance of inflammation

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[1] Russel on Indian Snakes, vol. i. p. 60.

round the punctured wound. Her temperature, ascertained before
the experiment, and now by thermometer *in cloacâ*, was found to
have fallen from 109° to 108°. In four hours she was much worse;
her breathing had become quick and laborious; venous blood
seemed to predominate in the circulation; the comb was bluish and
turgid.   There was very great prostration of strength; she was
unable to stand.   The sensorial functions were not apparently
deranged; the pupil was rather dilated than contracted; there were
no convulsions; no rigors.   Several liquid dejections occurred, and
some watery fluid was thrown up.   Her temperature was now re-
duced to 106°. I was obliged to leave the house; a servant watched
her; during my absence she expired, eight hours after she was
bitten.

I examined the fowl the next morning, ten hours after death. Ex-
ternally there was no appearance of swelling or of inflammation, or
of any kind of change, not even immediately on the spot wounded.
Beneath the skin, where the fangs had penetrated, there was much
cellular membrane and a layer of fat, sufficient to have prevented
the teeth reaching the muscle.   The cellular membrane under the
wound exhibited slight indistinct traces of inflammation.   On the
mucous membrane of the intestines there were a few red spots.
With this exception, the abdominal viscera in general had no unu-
sual appearance.   The brain exhibited no marks of disease.   Both
ventricles of the heart were empty, contracted and hard; the auri-
cles contained coagulated blood.   The lungs were unusually red,
and the air-cells were full of serum, which on pressure, flowed out
copiously.

Experiment 2.—Three days after, a powerful cock was exposed
to the same snake.   The snake fastened on his comb, and kept its
hold with its fangs for one or two seconds.   A little blood flowed
from the wound.

During the first four hours, the bird did not appear in the least
unwell, walking about and eating as usual.   After the fourth hour
he began to droop and lose his strength.   In eight hours he appeared
very weak, could scarcely walk; his feathers were ruffled; his eyes
nearly closed; his breathing laborious.   In this state he continued
about twenty-four hours without eating: he voided much green ex-
crement.   His temperature was reduced from 109.75, which it was
before he was bitten, to 106°.   About the twenty-eighth hour, he
appeared a little better, but still refused to eat: this was in the even-
ing; the next morning (he was in my bed-room in a large basket) I
was awakened by his loud and vigorous crowing.   He now appeared
recovered; he had regained his appetite, and his temperature had
risen to 108°.

Experiment 3.—The day following, the same snake was tried on
two puppies, both of whom it bit.   In neither instance was the
health deranged; nor had its bite, sixteen days after, any effect on
a small full-grown dog.

Experiment 4.—The snake used in this experiment, was of the same size nearly as the last;—it had been recently taken and was active and vigorous. On the 13th of February, 1817, a young cock, about half-grown was presented to it, and bitten on the breast;— the wound was so very slight, that it was only just perceptible. During the first hour the bird walked about and ate, as if nothing ailed him. Then he gradually sickened; his feathers became ruffled, he ate very little, and remained stationary in one place. For a day and half, he continued growing slowly worse; on the morning of the second day, he was found lying down, breathing very quick, and apparently insensible. He expired about the forty-third hour from the time he was bitten.

On dissection, no diseased appearances could be detected, excepting, perhaps, in the lungs, which were a little redder than natural.

Experiment 5.—A few minutes after the last fowl was bitten, another young cock, of the same age nearly and size as the preceding, was exposed to the snake. The snake fastened on his thigh, and inflicted rather a severe wound, from which some blood flowed. The cock became instantly lame, and in less than a minute was unable to stand. In about five minutes, his respiration became hurried and rather laborious; some alvine dejections took place. In about ten minutes he appeared to be in a comatose state; and for about five minutes he continued in this state—his respiration gradually becoming more feeble and laboured. In seventeen minutes, when his breathing was hardly perceptible, he was seized with a convulsive fit, which, in the course of the next minute, returned four or five times, each less violent than the former—and the last proved fatal.

The heart was immediately exposed: the auricles were found still acting. The lungs were redder than usual, and turgid with blood and serum. The brain, carefully examined, displayed no morbid appearance. The thigh bitten was slightly swollen; the skin surrounding the wound and the subjacent muscle were livid.

Experiment 6.—On the 29th of October, 1816, at Colombo, I assisted my friend, Mr. Finlayson, in examining a small hooded snake about two feet long, very soon after it was killed. We opened the poison-bag and tried the effects of its poison on a wild dove.

First, a little of it was applied to the conjunctiva of the eye. Immediately the eye-lid fell, and the eye closed as if from paralysis. The eye forced open—the pupil appeared much dilated.

Next, the tongue was punctured with a lancet dipped in the poison. In a few seconds, the bird had lost the use of its legs; it fell, and in two or three minutes expired, without apparent suffering or any remarkable symptom.

The body was immediately examined. No visible lesion could be discovered in any of the organs.

According to the best of my recollection, it was on this occasion that both my friend and myself tasted the poison of the Naia, applying a very minute quantity which had been taken from the poison-

11—g   18 *dav*

bag, the same as was used in the preceding experiments, to the tip
of the tongue. It tasted slightly acrid: the impression was of short
continuance; and besides this sensation no other effects were pro-
duced.[1]

2.—On the poison of the carawilla (trigonocephalus hypnale, Schlegel.)

Experiment 1.—The snake used in this experiment and the fol-
lowing, was about a foot long, just taken and very active. At Co-
lombo, in December, 1816, it bit a puppy, about two months old, in
two places—the side of the face, and the foot of one of the hind
legs. Immediately, judging from his howling, the animal seemed
to suffer much pain; he ran away, when liberated, on three legs,
making no use of the wounded leg. A drop of blood came from
each wound. In less than two minutes, the parts wounded began
to swell, and to discharge a thin reddish-brown ichor. In an hour
the swellings were very considerable, and the puppy was severely
unwell—crying piteously, lying down, and, when roused, hardly
able to stand. In about twenty-four hours, the swelling had ex-
tended to the parts adjoining the wounds, having subsided where
it first appeared. The wounds still discharged an ichorous fluid,
and appeared slightly livid. The health of the dog was improved;
he was able to eat and move about on three legs. In about forty-
eight hours, the swelling had nearly disappeared, and the animal
seemed to have recovered his health. The wounds had sphace-
lated extensively and deeply, and a purulent discharge had com-
menced. In twenty-four hours more, a slough had separated from
the wounds, which were now large deep ulcers, of a healthy ap-
pearance, promising to heal readily by the common process of
nature.

Experiment 2.—On the same day, another puppy was bitten by
the same snake in the fore-foot. The parts immediately swelled,
and discharged an ichorous fluid. From the foot the swelling ex-
tended to the shoulder; and from the shoulder to the integuments
of the chest; subsiding where it first appeared, as it spread beyond.
The health of the dog was much deranged; for two days it could
stand with difficulty, and ate very little. It had frequent small
bloody evacuations, as if the bowels were inflamed. At the end of
three days there was a considerable improvement; a great part of

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[1] This last experiment was not described in my work. In 1821, my friend
was alive and actively engaged in scientific pursuits in India. Unfortunately
for science, he was prematurely cut off, shortly after, on his homeward voy-
age, by pulmonary disease, contracted in Ava, where he accompanied, as
naturalist and medical officer, the political mission under the direction of
Mr. Craufurd, the able historian of the Eastern Archipelago. Mr. Finlay-
son's Journal of the Expedition, published after his death, bears ample and
equal proof of his zeal, talent for observation and reflection, and rich acquire-
ments.

the skin of the leg bitten was now livid, and on the fourth day sloughed off, exposing an extensive healthy ulcerating surface. On the fifth day, the animal was still sickly; the ulcer healed slowly, and the dog gradually recovered its health.

Experiment 3.—On the day following that on which the two preceding experiments had been made, a half-grown fowl was bit by the snake just above the left eye. The eye immediately closed; a watery fluid, like tears, flowed from it, and there was a similar discharge from the nostrils. When the eyelid was forced open, the pupil contracted readily on the admission of stronger light. The opposite side of the face soon became considerably swollen. The fowl drooped, but never lost the use of its legs. It refused to eat, and was much purged; what was voided had a chalky appearance. On the third day, it seemed to recover a little, and ate a small quantity of grain. On the fourth day, it was found dead.

Under the skin of the part bitten there was some coagulated blood, and the cellular membrane was discoloured. On the external surface of the heart, within the pericardium, there was a covering of coagulated lymph, of a reticulated appearance, strongly indicating recent inflammation of the organ. The lungs were rather redder than natural. The gall-bladder was distended with green bile. The intestines were not inflamed, and the other viscera had a healthy appearance.

Experiment 4.—On the day following, another fowl, half-grown, was bitten by the snake in the comb, which bled pretty freely. The comb and the skin of the head swelled slightly. For twelve hours the fowl was sickly, and ate very little. The next day the swelling had disappeared, and the fowl was well.

Experiment 5.—After a month's confinement, during which time the snake ate nothing, it appeared to be as active and as fierce as when first taken. It now bit a fowl, half-grown, on the side of the face, about a quarter of an inch below the eye. The surrounding skin immediately began to swell, and the eye to discharge a copious watery fluid. In a few hours the eye was greatly swollen and distended with effused coagulated blood. During the first twelve hours, the fowl seemed sickly, drooped, and ate nothing. It had numerous white alvine evacuations. In about twenty-four hours, its appetite returned, and it seemed pretty well, notwithstanding the diseased state of the eye, the sight of which it had lost from ulceration.

Experiment 6.—After another fortnight, spent in fasting, and without apparent diminution of activity; the snake bit a large frog on the head. The skin became slightly swollen, and discharged a bloody sanies. The swelling extended from the head to the trunk. The animal died in about five hours.

3.—*On the poison of the tic-polonga (vipera elegans, Daudin.)*

Experiment 1.—This and the following experiments were made with a tic-polonga, about four feet and a half long, and very thick. It had just been taken, and was in full vigour.

In February, 1816, at Colombo, the same fowl was exposed to this snake that had lost an eye from the bite of the carawilla. It seemed desirous to avoid the fowl, retiring and hissing with extraordinary shrillness and loudness; after being irritated very much, it darted at the fowl and struck it, but did not appear to have wounded it, though it really had, in the slightest manner possible, near the insertion of the great pectoral muscle. In about a minute the fowl was seized with violent convulsions, which, in two or three seconds, terminated in death.

The chest was immediately opened. The two auricles, and the right ventricle, and the great veins and arteries, were distended with coagulated blood. When first exposed, the heart was motionless; after the removal of the pericardium, the auricles exhibited, for a few seconds, a slight tremulous action. In the brain, lungs, and other viscera, no diseased appearances could be detected. The vessels generally were distended with coagulated blood. The muscles were very flaccid, and could not be stimulated to the feeblest contraction, immediately after death; even the pectoral muscles, when divided, showed no signs of irritablity, nor did the intestines. Where the wound had been inflicted, the skin and the muscle underneath were a little darker than natural, as if from a minute portion of extravasated blood. The muscular fibre was extremely soft and weak; slight pressure on the muscles occasioned the exudation of a little watery fluid, and some minute globules of air. There was not the slightest swelling of the part, or appearance of local inflammation.

Experiment 2.—About half an hour after the preceding experiment, a full-grown fowl was exposed to the snake. Even more provocation was required in this instance than in the first, to excite the snake to act;—at length he bit the fowl in the wing; the fangs penetrated the loose skin, and drew a little blood. At the moment, the fowl did not appear to suffer any pain, or to be in any way affected. In about a quarter of a minute, by a second-watch, its breathing had become accelerated; the eyes nearly closed, and the pupils a little contracted. In a minute, the fowl was seized with convulsions of a very severe kind,—the head was bent down and fixed on the breast; the legs were drawn up; in brief, every muscle appeared to be in violent action, and spasmodically contracted. The convulsions lasted about half a minute, when the fowl expired.

The appearances on dissection were the same as in the preceding instance. There was no discoloration or apparent change in the parts surrounding the wound.

Experiment 3.—Six days after, the snake bit a young dog, nearly full-grown, in the hind leg. A good deal of blood flowed from the wound. The dog immediately ran away, howling, with the foot drawn up.

During the first *ten* minutes, he was very restless; his movements were rather convulsive, particularly the motions of the hind leg that was not bitten. Sometimes he would lie down and appear a little composed, and as suddenly start up and re-commence a piteous howling.

In about *fifteen* minutes his breathing became hurried, the muscles became spasmodically affected, and violently so, particularly those concerned in respiration; copious evacuations took place, *sursum et deorsum.* His strength was now much diminished; he lay on the ground breathing rapidly and crying shrilly, every now and then starting up, as if from pain, and almost instantly falling again, as if from inability to support himself. In about *twenty* minutes, he was apparently almost exhausted; the breathing was short and spasmodic, as it were by jerks, and amazingly rapid— about ninety in a minute; each expiration was accompanied with a shrill sound, and now and then there was a full inspiration, followed by a deep groan. Even now, the sensorial powers seemed to be little affected; the poor animal was conscious of what was passing, and when patted on the head seemed to be soothed.

In about *twenty-six* minutes he became insensible and apparently comatose, not to be roused by any means of excitement applied. The pupils were rather contracted; respiration was quick, but not so quick as before, and more full: now and then it was more hurried, now and then there was a deep inspiration and a moan; his extremities were nearly cold.

In about *fifty-one* minutes his respiration became spasmodic, the head pulled down at each inspiration. The respiration became slower and slower, and gradually more feeble, till at the *fifty-eighth* minute it ceased entirely. The pupil was dilated; just before death there was a slight convulsive motion of the whole body.

The body was immediately examined. A good deal of serum was found effused on the surface of the brain, in its ventricles and at its base: the surface of the brain (the pia mater) was redder than usual; there were air-bubbles in the vessels of the pia mater, and in the larger veins. The liver was very red, and gorged with blood; the lungs were rather red, condensed, and contained little air. The ventricles of the heart were distended with blood, the right ventricle especially; the right auricle was less distended, the left auricle was empty. The blood in the heart and veins was liquid; it did not coagulate on exposure to the air. There were red spots on the inner coat of the stomach. The other viscera exhibited nothing unusual. The foot bitten was very slightly swollen, owing to the effusion of a little serum and blood. The muscle under the wound was nearly black, there were air-bubbles in the

adjoining cellular membrane; there was no offensive smell from it: the body was still warm.

Experiment 4.—Thirty-four days after the last experiment, during which time the snake would eat nothing, it bit a rat. The animal immediately lay down motionless; its respiration became quick and convulsive; and after two or three slight convulsions of the body in general, it expired.

The body was immediately opened. The heart had ceased to contract; the auricles did not contract when punctured. The surface of the heart was unusually red and vascular, as if inflamed; its cavities were distended with blood. The lungs were rather redder than natural. The muscular fibre in general had entirely lost its irritability. There were no marks of disease amongst the abdominal viscera.

The first experiment with this snake was made on the day it was brought to me, the 3d of February; the last, which I have now to describe, on the 27th and 28th of June, an interval of 146 days, which it had passed fasting. It had refused different kinds of food offered, and yet it looked now as well as ever, and appeared to be equally vigorous; and from the result of the following experiments, its poison seemed to be not exhausted or weakened, but concentrated and more terribly active. As I was obliged to leave Colombo at this time, I do not know how much longer the snake lived without eating.

Experiment 5.—On the 27th June, the snake bit a full-grown fowl in the face. The wound was inflicted in an instant; the fangs were instantly disengaged. In a few seconds the fowl was convulsed; every muscle seemed to be thrown into violent spasmodic action; the head was drawn down on the breast, the legs were extended, and the animal, lying on its belly, was moved about rapidly and irregularly by a quick succession of those involuntary spasmodic actions of its muscles, till it expired, which it did in rather less than a minute from the instant it was wounded.

The body was immediately opened. The muscles divided by the scalpel did not show the slightest signs of irritability, nor did the intestines; the heart contracted very irregularly, and so very feebly, that it had no effect in propelling its contents. The auricles were gorged with blood, particularly the right; the left ventricle was empty, and the right contained only a small quantity of blood; the arteries and veins were full of blood, which had coagulated firmly, even in the minute branches. There was no unusual appearance in the brain, or in any of the other viscera, excepting the distended state of their blood-vessels. A reddish sanies oozed from the wound; the skin round it was discoloured, without swelling; the muscle under the skin was blackish and tender, as if severely bruised; the adjoining cellular membrane was slightly emphysematous.

Experiment 6.—On the following day, the snake wounded another full-grown fowl, inflicting with its fang, in the side of the face, a puncture only just perceptible. During the first minute and a half, the fowl did not appear to suffer in the least, when it was seized with violent convulsions, which proved fatal in about fifteen seconds.

The appearances on dissection, which was commenced immediately, were very similar to the last. The action of the heart was perhaps a little less feeble; the vermicular motion of the intestines had not entirely ceased, and the blood was not quite so firmly coagulated. All the cavities of the heart were empty, with the exception of the right auricle, which was distended with blood.

Conclusions and general remarks.

From the experiments on the poison of the hooded snake, it would appear that its bite is not necessarily fatal to fowls; that the effect of the bite varies a good deal according to circumstances which it is not easy to calculate; that the poison is capable of being soon exhausted; that the symptoms produced by the poison, though not uniformly the same, pretty generally correspond; and that, in conjunction with the appearances on dissection, they seem to indicate that the lungs are the *principal* seat of the diseased action.

From the experiments on the bite of the carawilla, it would appear that it is rarely fatal to small animals; that its poison is not easily exhausted; that the symptoms produced by it are pretty uniform; that they are different from those produced by the poison of the hooded snake, the deceased action being more local and much more inflammatory—commencing in the part bitten, spreading, progressively, losing its force as it extends, and probably never proving fatal, except it reach a vital organ.

From the experiments with the tic-polonga, it appears that its bite greatly exceeds in fatality that of the hooded snake, or of the carawilla, and that the action of its poison is different and peculiar. Judging from the symptoms and appearances on dissection, its poison seems to exert its influence primarily and principally on the blood and muscular system, tending to coagulate the former and convulse and paralyse the latter. All the experiments seem to point directly to this conclusion, not excepting even the third, the peculiarities of which may be referred to the reaction, which it may be conceived took place, the animal being pretty large and strong, and not overpowered by the immediate effect of the poison.

From the whole of the preceding experiments, it may be inferred, reasoning from analogy, in relation to man, that the bite of the tic-polonga is most dangerous; that of the hooded snake is dangerous in a less degree; and that of the carawilla, least of all, and probably never fatal. The result of the inquiries which I made amongst the natives, though not very satisfactory, tended to confirm these conclusions: I found them generally of opinion, that the bite of the tic-

polonga is unavoidably fatal, but that the hooded snake only occa-
sionally so.   Perhaps they exaggerated a little in maintaining the
first part of this statement: the latter part of it I believe to be quite
correct; for I have seen several men who had recovered from the
bite of the hooded snake, and I have heard of two or three only to
whom it had proved fatal.

I regret that I have nothing original to offer respecting the medi-
cal treatment of the bites of these snakes.   It was my intention to
have made a series of experiments on the subject; indeed, the expe-
riments which I have detailed were merely preliminary to that
inquiry, being instituted to smooth the way, to determine, if pos-
sible, the mode of action of each poison, and furnish data for
inferring what are the pure effects of the poison, what of the
powers of nature opposing its effects, and what those of the me-
dicine administered.   In no subject is discrimination more re-
quired than in this mysterious one; and in no one perhaps has
less judicious discrimination been used.   It has often been taken
for granted that the poison of all snakes is similar—not differing in
its kind, but only in its intensity of action; and, agreeably to this
assumption, that the medicine useful in one instance must be ser-
viceable in all.   And, too often, medicines have got into repute as
antidotes from being given in slight cases, in which recovery would
have taken place without medical treatment—beneficial changes
that were due merely to the preservative powers of the constitution.
The reputation that many Indian medicines, and especially that
snake-stones have acquired, afford striking proof of the preceding
remarks; of three different kinds of these stones which I have ex-
amined, one consisted of partially burnt bone, another of chalk, and
the third principally of vegetable matter; this last resembled a bezoar.
All of them (excepting the first, possessed of a slight absorbent power)
were quite inert, and incapable of having any effect, exclusive of
that which they might produce as superstitious medicines, on the
imagination of the patient.[1]

The probability is, that the poison of each different kind of snake
is peculiar; and that, when fully investigated, the effects of each will
be found to require a peculiar mode of treatment, the nature of which
can only be ascertained by actual experiment.   In the mean time,
I may remark, that fortunately for man, in this great obscurity, the
immediate treatment of all poisoned wounds, merely locally con-
sidered, is simple and very similar.   The obvious indications are,
to extract the poison as much as possible, and as speedily as possi-
ble; and to endeavour to prevent its entering the circulation.   The
first indication is best accomplished by cutting out the part bitten,
and scarifying the surrounding integument.   If the person bitten

[1] It is the opinion of the natives that these stones are found in the brain
of snakes.   From Sir Alexander Johnson, to whom I was indebted for the
specimens I examined, I learnt that those of the first kind are manufactured
by the monks of Manilla, who carry on a lucrative trade in them with the
merchants of India.

want courage to do this, he should suck the wound well, and after-
wards apply caustic, if at hand. The second indication may be
tolerably fulfilled (if the part bitten admit of the application of a
ligature) by tying a handkerchief very tightly just above the wound.
Respecting the employment of oil, arsenic, and eau de luce, having
no experience of their effects, I can offer no decided opinion.   Oil
seems to have been useful, both applied externally, and taken inter-
nally, in many instances of the bite of the viper; and arsenic seems
to have done good in some instances of the bite of the hooded snake.
Eau de luce does not appear to have deserved the high character
that was first given to it, indeed I am not aware of any satisfactory
proof of its ever having been beneficial.

XXIII.

ON THE STRUCTURE OF THE HEART OF BATRACHIAN ANIMALS, ESPECIALLY OF THE GENUS RANA OF LIN-NÆUS.

From the urinary organs of reptiles, my attention, whilst I was
in Ceylon, was directed to other parts of their structure, and espe-
cially to the apparatus of circulation and respiration.

It was at that time commonly asserted by the highest authorities
in comparative anatomy, and generally believed, that the animals
belcnging to the genus rana, and indeed all those included in the
natural order of batrachians of Cuvier, in the conformation of their
heart, differed from the other reptiles, which possess a bi-auricular
heart; and resembled fishes—which have only a single auricle as
well as ventricle.

The close analogy in general structure which exists between the
batrachians and the other reptiles, led me to doubt the accuracy of
this conclusion; and observations which I had an opportunity of
making first in Ceylon, and afterwards in 1824 and 1825, in the
Ionian Islands, satisfied me that my doubt was well founded.

Selecting old and large animals, especially the toad, which in the
Ionian Islands often attains a great size, I experienced but little dif-
ficulty in demonstrating the bi-auricular structure of its heart.   It
was best effected by making an incision into the ventricle, and in-
flating the organ with the blow-pipe.   By this method, with the aid
of fine probes, manipulating under water, it was clearly proved that
the heart of the animal has two auricles, which are divided by a
transparent membranous septum, possessing fibres, which appear to
be muscular; that they communicate with the ventricle by a com-
mon and very short passage, and that they have no communication
with each other, excepting in the passage above the valves common
to both.

The same fact as to structure, I found might also be demonstrated, by blowing air through either of the two pulmonary veins which return the blood from the lungs to the heart; or, by inflating the sinuses in which the venæ cavæ terminate. By inflation of either of the pulmonary veins, the pulmonic auricle was distended, and not the systemic. By inflation of the sinuses of the venæ cavæ, the contrary effect was produced, showing, at the same time, that the margin of the septum was qualified to act the part of a valve, and prevent the blood of one auricle from passing into the other.

But even supposing the margin of the septum not fitted to perform the function of a valve, I apprehend, from what I have observed in watching the heart's action, that the blood from one auricle would not pass into the other, the contraction of the two auricles being synchronous, the auricles first contracting, next the body of the ventricle, and lastly that part of the ventricle of a conical shape, which may be considered almost as a second ventricle.

This last mentioned part (to notice it more particularly, as I believe it deserves) appeared from my dissections, to be separated from the body of the ventricle by three valves, of a semilunar form, one large and two small, and to have attached within it, to the side of its cavity, a pale fleshy projection, a moveable septum, above which it gave origin to four great arteries, namely, two pulmonary arteries and two aortæ, the latter considerably larger than the former, each provided with its own valve. The action of this part is not a little curious: when I have watched it, it did not appear to contract simultaneously, as a whole, but one-half first, and then the other, as if intended, in conjunction with the various anastomoses of the arterial system, to preserve a constant, though small and feeble current of blood, to supply all the parts of the body according to their various demands.

The same general structure of heart, I have observed in the bull-frog distinctly, and in the common frog also, especially its double auricle. Whether it exists in all the other species of the genus, I have not ascertained; most probably it does. And reasoning from analogy, it appeared highly probable, that all the other genera of the order batrachia have a similar conformation, both of this vital organ, and of the sanguineous system in general.

I then remarked, should this analogical inference prove correct, and its truth be established by observation, these animals in their mature state would no longer be an anomaly in the classification of reptiles on account of their hearts, and they would still continue as a link connecting the reptiles with fishes—the perennibranchiate, through the whole of their existence; the other species during the first stage of it, by the peculiarities of their respiratory organs.

This conjecture was offered in 1825, in a paper which was presented to the Royal Society, and which was afterwards published in the Edinburgh Journal of Science, edited by Professor Jameson; and I am happy to have to state, that it has been amply verified by

the able and minute researches of Mr. Owen, as well as by those of other inquirers.[1]

I have stated, that when I began my inquiries, and first published my observations, all the batrachians were considered by the ablest comparative anatomists from the time of Harvey, downwards, as analogous to fishes in the auricular structure of the heart. And, I believe I was fully justified in the conclusion, by reference to their writings.[2] Since then, however, I have learnt, that our distinguished countryman, Mr. Hunter, was acquainted with the fact, that some of the batrachians have a bi-auricular heart, and that he made it in part the basis of a subdivision of the order. This I was informed of by Mr. Owen, who had the goodness to show me a highly interesting document, preserved through the medium of a copy, a classification by our great physiologist, of the animal kingdom according to its organisation, and which Mr. Owen has in part given in his valuable prefatory remarks on the physiological labours and discoveries of Hunter, prefixed to his edition of this portion of his works.

. At what time Hunter ascertained the fact, it is impossible now to determine, in consequence of the unfortunate destruction of his MS. papers: it is most likely that it was late in life, as there is no preparation in his splendid collection, in the Museum of the College of Surgeons, illustrative of it;—the only two preparations of the heart of the common toad and frog, left by him, seem from the descriptive catalogue published in 1831, designed to illustrate the commonly received idea.[3]

The circumstance that Hunter was acquainted with the bi-auricular structure in question, may serve to account for the adoption of it by Sir Everard Home. In his "Systema Régni Animalis," in the third volume of his Lectures on Comparative Anatomy, he includes the genus Rana in the same class with lizards and snakes, to which he assigns "Cor uniloculare, bi-auritum, sanguine frigido

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[1] Mr. Owen's observations are to be found in his paper on the Structure of the Heart in the Perennibranchiate Batrachia, communicated to the Zoological Society, April, 1834.

[2] According to Harvey, "Piscibus, serpentibus, lacertulis, testudinibus, ranis et hujusmodi aliis, tum auricula, tum cordis ventriculus unus." Exercitatio Anatomica de Motu Cordis, cap. xvii.
According to Cuvier, "Les Betraciens n'ont au cœur q'une seule oriellette et un seule ventricule." Le Régne Animal, nouvelle édition, Paris, 1829.

[3] The following is the account given of two preparations alluded to in the text:—"175. The heart of a large toad (Rana Bufo) injected. It is composed of a single auricle and ventricle; from the latter, the arterial trunk or aorta arises, and immediately after its origin divides into two branches, which ultimately unite into one descending aorta, at the inferior part of the spine. Hunterian."
"176. The heart of a frog (Rana temporaria) injected. As in fishes, the heart is composed of two cavities, a single auricle, and a ventricle. Hunterian."
Catalogue of the Contents of the Museum of the Royal College of Surgeons in London, Part V. 1831, p. 16.

rubro;"—but without specification there, or in any of his writings, of the observations by which the fact was determined.

XXIV.

AN ACCOUNT OF SOME EXPERIMENTS ON ANIMAL HEAT.[1]

In the present uncertain state of our knowledge of animal heat, theoretically considered, three circumstances are particularly deserving of attention, viz. the relative capacities of venous and arterial blood for heat, their comparative temperatures, and the temperatures of different parts of the animal body.

On the first of these subjects, we possess only the experiments of Dr. Crawford, which I believe have not yet been repeated, notwithstanding they form the basis of his hypothesis.

On the second, little inquiry has been made, and especially of late years, since the improvement of the thermometer.

And, on the third, the observations that have been collected are very few in number, and with the exception of those of Messrs. Hunter and Carlisle, are scarcely deserving of confidence.

Such were the inducements that led me to the consideration of each of these subjects apart, and to endeavour to acquire by experiment some more certain knowledge respecting them. The experiments that I have made will be described in the two following sections, and in the last will be offered the few remarks and conclusions which naturally arise, and are fairly deducible from the results.

1.—*On the capacities of venous and arterial blood for heat.*

I must premise, that my object has been to endeavour to ascertain the relative capacities of venous and arterial blood for heat, rather than their exact specific caloric. The latter, from many circumstances, is difficult to be accomplished; whilst the former is comparatively easy, and, in a theoretical point of view, is probably equally useful.

I have employed both the methods commonly used. I shall mention most of the experiments that I have made, without noticing the repetitions of them, and shall begin with those on the times of cooling of equal volumes of venous and arterial blood.

The blood used was from the jugular vein and the carotid artery of a lamb about four months old. It was received in bottles, and, to remove the fibrin, which is a great impediment in experiments

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[1] This paper first appeared in the Philosophical Transactions for 1814; most of the prefatory remarks are still applicable.

of this kind, it was immediately stirred with a wooden rod.   In respect to colour, the difference between the venous and arterial blood was not so great as in the sheep's, and this in a great variety of instances I have always observed, the venous being of a less dark hue.   The specific gravity of the venous blood, without the fibrin, was found to be 1050, and that of the arterial 1047.

A glass bottle, equal in capacity to 2518 grains of water, and weighing 1332 grains, was filled respectively with water and venous and arterial blood of the temperature of the room 62°, about four hours after the blood had been drawn, during which time each bottle had been closely corked.   A delicate thermometer, by means of a perforated cork, was placed in the middle of the liquid.   The bottle was then plunged into water of the temperature 140 Fahrenheit, and when the mercury had risen to 120, the bottle was quickly wiped, and suspended in the middle of the room ; and the progress of cooling was noticed every five minutes till the thermometer had fallen to 80°. The following were the general results obtained :

> Water cooled from 120° to 80° in 91 minutes.
> Arterial blood in   -   -   89   —
> Venous blood in   -   -   88   —

Considering, therefore, the capacity of water for heat to be denoted by 1000, neglecting the effect of the glass bottle, producing a difference only of about half a minute, and the same in each instance, and dividing the times of cooling by the specific gravity, the relative capacities of venous and arterial blood, without fibrin, appear to be as 921 and 934.

In the following experiments, the same kind of blood, and the same quantity, was used as in the preceding.   The mixtures were made in a very thin glass receiver, containing a very delicate thermometer.   The temperature of the room was 66°.

Hot water, temperature 121°, cold water 61°.   Mixture of the two 90°, after two minutes 89°, after three 88°, after eight 87°.

Venous blood 121°, water 62·5, mixture 89°, after three minutes 88·5, and after seven 87°.

Now, allowing about one degree of the cooling effect to have been produced by the receiver, indicated by the admixture of the hot and cold water, calculating the quantity of blood used from the knowledge of its volume and specific gravity, employing the formula given by Professor Robinson, which consists in multiplying respectively the weight of the water and the blood by the change of temperature, and dividing the first product by the second, the quotient or specific caloric for venous blood appears to be as 812, and for arterial as 814, results very similar to those I have obtained with the blood of the sheep.

In the remaining experiments, blood with the fibrin present was employed, and with this exception, they were perfectly similar to those already described.

The blood, used to ascertain its time of cooling, was obtained

from a sheep; and one day the vein was opened, and on the next
the artery. The capacity of the bottle employed exceeded that of
the first by one ounce measure of water; but it was equally thin.
The air of the room was of temperature 69°.

Water cooled from 120° to 80° in 118 minutes.
Venous blood in   -   -   112   -
Arterial blood in   -   -   113   -

And hence, as the latter was of specific gravity 1049, its capacity
for heat seems to be as 913; and as the former was of specific gra-
vity 1051, its capacity appears to be 903.

In the following experiment, equal volumes of fluid blood and of
water were used, which was easily accomplished by means of a
thin bottle with a large mouth, to which a cork was adapted, with
a perforation more than sufficiently large to admit the bulb of a
very delicate thermometer, and of course to allow, when the bottle
was filled to the brim, the excess to flow out on the introduction of
the cork, which was always similarly placed. To retard the pro-
cess of cooling, the bottle was closely surrounded by a thick layer
of what is commonly called cotton wool. Its capacity was equal
to five ounce measures of water, or 2400 grains. It was first filled
with cold water, which, when its temperature had been ascertained,
was thrown into the receiver before used; it was next filled with
hot water, of temperature about 110°, so that the heat of the glass
might be nearly the same as that of the blood; and, lastly, when
the vein or artery had been opened, the bottle was immediately
emptied and filled with blood—the temperature of which was ascer-
tained by the thermometer in less than a quarter of a minute. The
mixture was now instantly made, and by the same thermometer the
highest temperature of the mixture was discovered.

The four following trials were made on the blood of two lambs,
both about five months old. The temperature of the air was 60°.

Cold water 57·5. Venous blood 100°. Mixture 80°; after one
minute 78·5.

Water 58°. Arterial blood 103°. Mixture 80°; after one mi-
nute 79°.

Water 58°. Venous blood 101·5. Mixture 79°; after one mi-
nute 78·25.

Water 57°. Arterial blood 106·5. Mixture 81°; after one mi-
nute 80°.

The rate of cooling was not noticed after the first minute had
elapsed, as the blood then generally began to coagulate. The spe-
cific gravity was only ascertained in the two last trials; that of the
venous blood was found to be 1050, and that of the arterial 1049;
and hence, allowing, as before, one degree of the cooling effect to
be produced by the receiver, the capacity of venous blood for heat
appears to be 852, and that of arterial blood 839.

It is evident that these trials admit of less accuracy than the pre-
ceding; and much more confidence, it appears to me, is due to the

third series of experiments, so that, if required, I should be inclined to give the numbers thence deduced as the greatest approximation to the truth.

2.—*On the comparative temperature of venous and arterial blood, and of different parts of the animal body.*

To endeavour to ascertain the comparative temperature of venous and arterial blood, I have made a considerable number of experiments; some of which, on lambs, sheep, and oxen, it will be sufficient for me in this place to describe. In each instance a long incision was made through the integuments, the jugular vein was laid bare, and the exact seat of the carotid artery found. The vein was then opened, and a small, delicate thermometer introduced, and thrust about an inch up the vessel beyond the wounded part, and as the bulb of the instrument was small, the flow of blood was not stopped. When the mercury was stationary, its height was marked. The carotid artery next was divided, and the thermometer was immersed in the current of blood, and left there till it ceased to rise.

The following are the results of five experiments made on lambs, all of which were about three months old. The thermometer in the shade stood at 65°.

	Venous blood			Arterial blood		
1.	Venous blood		102·5	Arterial blood		104°
2.	-	-	104°	-	-	105°
3.	-	-	104°	-	-	105°
4.	-	-	103·5	-	-	105°
5.	-	-	104°	-	-	105°

The following results were obtained from three experiments on sheep, the exact age of which I could not ascertain. The thermometer in the shade was 60°.

	Venous blood			Arterial blood		
1.	Venous blood		103·5	Arterial blood		104·5
2.	-	-	102·5	-	-	104°
3.	-	-	103°	-	-	104°

The experiments on oxen were only two in number. The temperature of the air was 64°.

	Venous blood			Arterial blood		
1.	Venous blood		100°	Arterial blood		101·5
2.	-	-	100°	-	-	101°

In both instances the oxen were knocked down before the vessels were opened; and as respiration had ceased in consequence of the injury of the brain and spinal cord, no difference of colour, of course, was perceptible between the blood from the jugular vein, and that from the carotid artery.

These results, so different from what might have been expected, from the observations of Messrs. Coleman and Cooper, on the temperature of the two sides of the heart, led me to repeat their experiments. The experiments in which I place most confidence, were

made on lambs about four months old, and to these I shall confine myself at present. In each instance the animal was killed by the division of the great vessels of the neck: an opening was made immediately into the thorax, and a very delicate thermometer was introduced into the ventricles of the heart by means of a small incision. The operation occupied so short a space of time, that in three instances the right auricle had not ceased contracting.

1.	Venous blood	-	-	-	104°
	Arterial	-	-	-	105·5
	Rectum	-	-	-	104°
	Right ventricle	-	-	·	105·5
	Left	-	-	-	106°
2.	Rectum	-	-	-	105°
	Right ventricle	-	-	-	105°
	Left	-	-	-	106°
3.	Rectum	-	-	-	105°
	Right ventricle	-	-	-	105·5
	Left	-	-	-	106·5
4.	Rectum	-	-	-	105°
	Right ventricle	-	-	-	106°
	Left	-	-	-	107°

I cannot well explain the difference which exists between the results of the preceding experiments, and those of Messrs. Coleman and Cooper, which are directly opposite, excepting on the supposition of its being connected with the mode in which the animals they experimented upon were killed, viz. by asphyxia. In death by asphyxia, there is generally an accumulation of blood in the right ventricle, and in many instances I have observed, when the right ventricle has been distended with blood, little difference of temperature between the two sides of the heart.[1]

To describe all the experiments that I have made to ascertain the temperature of different parts of the animal body, appears superfluous, especially as the comparative results were very similar. It will be sufficient, therefore, here to notice the observations made on the human body, and on that of a lamb.

That the thermometer might be equally applied to all parts of the surface, its bulb, in form nearly cylindrical, was fixed to a small piece of cork, hollowed and lined with fine wool, and thus half its superficies was applied in each instance. The observations were made on the naked body at seven A. M. immediately after quitting

[1] In instances of death by asphyxia, theoretically considered, it might be expected, that the blood returning from the lungs to the left side of the heart not having been acted on by air, and there having been no production of heat, would necessarily resemble venous blood, in temperature as well as in colour. In this point of view, the results of the experiments referred to above may be considered confirmatory of the hypothesis in which the lungs are held to be the chief seat of the heating process.

bed. The temperature of the air of the room was 70°. The following were the results obtained.

At the central part of the sole of the foot	90°
Between the malleolus internus and the insertion of the tendo achillis, where the artery is felt	93°
Over the middle of the tibia	91·5
Over the middle of the calf	93°
Over the popliteal artery at the bend of the knee	95°
Over the femoral artery in the middle of the thigh	94°
Over the middle of the rectus muscle	91°
Over the great vessels in the groin	96·5
About a quarter of an inch below the umbilicus	95°
Over the sixth rib, on the left side, where the heart is pulsating	94°
Over the same place in the right	93°
Under the axilla, the whole surface of the bulb being applied	98°

About an hour had now elapsed from the commencement of the experiment. The thermometer again applied to the sole of the foot rose no higher than 85°, five degrees less than at first. A disagreeable sensation of cold was experienced, and particularly in those parts not supplied with large vessels, and out of the course of the great arteries. The body remained unpleasantly chilly till breakfast had been taken, and then a slight degree of pyrexia was perceived; the heat of surface being increased, the pulse quickened, and the mouth slightly parched. After breakfast the thermometer was applied to both hypochondriac regions, and the left was found one degree higher than the right.

To ascertain the temperature of different parts of the surface beneath the integuments, the bulb of a thermometer was introduced through small incisions about half an inch between the skin and subjacent parts of a lamb just dead. The heat of the rectum was first ascertained, as a means of marking the rate of cooling, and the different parts were then tried in the following order:—

Venous blood in the jugular vein	105·5
Arterial blood from the carotid artery	107°
Rectum	105·5
Over the metatarsal bone	97°
Over the tarsal bone	90°
Over the knee joint	102°
About the head of the thigh	103°
At the groin	104°

Nearly a quarter of an hour had been occupied in making these observations, and the temperature of the rectum was now found to be 105°. The three great cavities were next opened in the order enumerated.

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Near the lower part of the liver -	-	-	106°
The substance of the liver	-	-	-	106·5
The substance of the lung	-	-	-	106·5
The left ventricle	-	-	-	107°
The right ventricle	-	-	-	106°
The central substance of the brain		-	-	104°
Rectum -	-	-	-	-	-	104·5

Surprised at the temperature of the brain being lower than that of the rectum, I was led to repeat the experiment. It may be proper to notice a few of the results, as it is a curious circumstance which they confirm. The four experiments I shall mention were made on lambs. As soon as the animal was dead, the cranium was perforated, and a delicate thermometer introduced into the central part of the brain.

1. Brain	-	104°	Rectum	-	104·75
2. Brain	-	105·75	Rectum	-	105·5
3. Brain	-	105·5	Rectum	-	106·5

4. Posterior part of the brain 105·5. Anterior 103°. Rectum 106·5.

The temperature of the air at the time was 68°. Different parts of the brain were found to vary considerably in temperature; the anterior, as already noticed, being lower than the posterior, and the superficial than the deep-seated parts.

3.—Remarks and conclusions.

That there is no material difference between venous and arterial blood in respect to specific caloric, excepting what arises from difference of specific gravity; that the temperature of arterial blood is higher than that of venous; and the temperature of the left side of the heart than that of the right; and lastly, that the temperature of parts diminishes as the distance of parts from the heart increases—are the general results of the preceding experiments.

Admitting the accuracy of these experiments, and I think that they will be found correct when repeated, what are their consequences in a theoretical point of view?

They are evidently in direct opposition to Dr. Crawford's hypothesis, the essence of which is, that the capacity of arterial blood for heat is greater than that of venous, that there is no difference of temperature between the two ventricles of the heart, and in fact that the heat of all parts is nearly the same.

They are more agreeable to, and even support, the hypothesis of Dr. Black, that animal heat is produced in the lungs, and distributed over the whole system by means of the arterial blood.

Neither are they inconsistent with that hypothesis which considers the production of animal heat as dependent on the energy of the nervous system, and arising from all the vital actions constantly occurring.

Besides the results of the preceding experiments, many arguments may be advanced in opposition to Dr. Crawford's hypothesis.

As we never perceive a difference of capacity in bodies without a difference of form or composition; and as very slight differences of the former result only from great changes of the latter, it might be expected, *à priori*, as no difference excepting that of colour has been detected between venous and arterial blood, that their specific caloric would be very similar. From analogy, also, it might have been expected that the capacity of arterial blood for heat would be much less than that of water, as water appears to exceed almost every other fluid, and as the capacity appears to diminish as the inflammability of compounds increases. But the strongest arguments against this hypothesis are to be derived from the experiments of MM. Delaroche and Berard.[1]

Dr. Black's hypothesis appears to me far more satisfactory than Dr. Crawford's, and capable of explaining a much greater number of phenomena.

The last hypothesis which I mentioned, that which refers animal heat to vital action, has many facts in its support.

It may be said that the viscera of the thorax and abdomen are of the highest temperature, because these parts are, as it were, the elaboratories of life; and that the heat of the arterial blood, and of the parts best supplied with this fluid, is greatest, because they lie deepest, and abound most in the principle of life or vital action. This explanation was suggested to me by my brother, Sir H. Davy. There are some facts which I have observed agreeable to it, but not more so than to the hypothesis of Dr. Black.

I have found the stomach of the ox, the pyloric compartment, of a higher temperature than the left ventricle itself; thus, when the latter immediately after death was 103°, the former, full of food, was 104·5. I have also found the temperature of young animals, in whom all the vital actions are most energetic, higher than that of animals arrived at maturity. I may mention here, in illustration of this statement, a few observations made on infants, as I am not acquainted with any yet published. In one instance I found the heat under the axilla of a child just born 98·5; after twelve hours 99°; and after three days the same, during the whole of which time it appeared in perfect health.

On five other children of the same age, similar observations were made. In two instances of weak infants, the temperature, one hour after birth, was found not to exceed 96°, which is two degrees below the standard heat of man in health; but their respiration was

[1] On the specific caloric of gases; from which it would appear that the capacity of carbonic acid gas for heat is actually higher than that of atmospheric air; or in other words, that the capacity of the air expired is greater than that of the air inspired; and consequently, in this point of view, respiration ought to be, as it was considered by the ancients, a cooling process.— *An. de Chim.* lxxxv.

still languid, and the next day the heat of the axilla had risen in one to 98·5, and in the other to 99°.

To conclude; as in each hypothesis examined, difficulties are found to exist from facts or the results of experiments of an unbending nature, we must at present either suspend theory altogether and search for *experimenta crucis*, or adopt that hypothesis which is conformable to the greater number of facts. The first measure is certainly most philosophical; but to the latter we are naturally most inclined; and if I were questioned which view is preferable, I should not hesitate in selecting Dr. Black's, which to me appears both most simple and most satisfactory.

XXV.

OBSERVATIONS ON THE TEMPERATURE OF THE BRAIN COMPARED WITH SOME OTHER PARTS OF THE BODY.

From the results of some observations already detailed (p. 286), it would appear that the temperature of the brain is not quite so high as that of the rectum, in the instance of sheep, and that the superficial portion of the brain is less warm than the deeply-seated.

Professor Müller, in his Elements of Physiology, has called in question the accuracy of these observations, and has rejected the conclusion to which they led.

I have been induced, in consequence, to make recently some additional trials on sheep, which I shall now relate.

1.—December 4th, 1837, air 44°. The head of a two-years-old wether was cut off, immediately after death by the division of the great vessels of the neck. A thermometer plunged into the base of the brain through the foramen magnum, rose to -   104°
Forced in further, so as to be near the surface of the cere-brum, it fell to -   103°
Now introduced into the rectum, it rose to -   105°

2.—An ewe two years old; thermometer in recto before being killed   106°
After decapitation, at base of brain -   105·5
     near the surface   104°

3.—January 22, 1838; temperature of air 45°. Immediately after decapitation, base of brain -   105°
Rectum   106°

4.—Before decapitation, rectum   103·5
After decapitation, brain -   103°

5.—Before decapitation, rectum,	-		-		-	104°
After	-	base of brain -		-	-	105°
-	-	near surface of cerebrum		-		103°
6.—Before	-	rectum	-		-	105°
After	-	base of brain -		-	-	105°
-	-	near surface of cerebrum		-		104°
7.—Before	-	rectum	-		-	104°
After	-	base of brain -		-	-	105°
-	-	near surface of cerebrum		-		104°

8.—Rectum, before decapitation - - -	105°
Venous blood; the thermometer placed in jugular vein, the blood flowing freely - - - -	103°
In current of arterial blood from carotid artery -	105·5
In current of mixed blood, from the great vessels of the neck divided in the ordinary manner - -	104°
Brain at base, after decapitation - - -	105°
Near the surface of cerebrum - - -	104°
Right ventricle (some blood in it) - - -	106°
Left ventricle (empty) - - .. -	108°
Liver - - - - - -	107°
Crystalline humour of the eye - -	102°

The subjects of these observations, of the 22d January, were virgin ewes between two and three years old, with the exception of No. 7, which was a wether.

These results are generally confirmatory of the preceding, although not without exception in relation to the base of the brain. In another place, further on, I shall have occasion to give some additional observations on this point.

That the temperature of the surface of the brain at least should be uniformly less than that of the rectum, appears to me what might be expected à priori, considering the comparatively small supply of blood to that part, and its exposure to cooling influences. In the instance of birds, the effect is very strikingly exemplified; thus, in the turkey, on the 22d February, when the thermometer in the open air was 34°, and the room 46°, in cloaca it was 108°, in the gizzard 109°, under the pectoral muscle 108°, and in the brain 90°. The temperature in cloaca was ascertained during life; the other observations were made as soon as possible after death; 1st, in the brain; 2d, under the pectoral muscle; and lastly, in the stomach.

The effect of cooling influences on the brain is shown in a very decided manner in the instance of the brain of man after death. Excepting the hands and feet, the brain appears to lose its heat more rapidly than any other part of the body. Some precise observations on the temperature of the brain after death, compared with that of other parts, warrant this remark: they will be given in detail in the sequel.

XXVI.

OBSERVATIONS ON THE TEMPERATURE OF MAN AND OTHER ANIMALS.[1]

In this place I shall give the results of some inquiries, which chiefly between 1816 and 1820, I instituted on the temperature of man and other animals,—a subject, in a physiological, and in relation to man, in a pathological point of view, deserving, I believe, of more minute attention than, to the best of my knowledge, it had then received.

1st. I shall describe the operations I have collected, to ascertain the variation of temperature to which man is liable, in passing from the temperate into the torrid zone; in descending from a cool mountainous district into a hot low country; and in inhabiting a region where the diurnal vicissitudes of temperature are considerable.

2dly. I shall give an account of the attempts I have made to ascertain the temperature of different races of men.

3dly. I shall relate the results of my experiments on the temperature of different kinds of animals.

And, I shall conclude, with drawing such inferences as the premises may seem to warrant, and with making a few remarks on animal heat, as a speculative question.

1.—*Of the variable temperature of man.*

In a voyage from England to Ceylon, in the year 1816, I had an opportunity of observing the effect of passing from one zone to another on the temperature of man.

It was in winter, in the month of February, that we set sail from England. I commenced my observations in March, when we began to experience the tropical heat; and on the 10th of the month, when our ship was in latitude N. 9° 42′, the weather fine, an agreeable breeze blowing, and when Fahrenheit's thermometer, exactly at noon, under an awning, where the passengers were assembled, was 78°. The gentlemen who were so obliging as to allow me to try their temperature, were all in good health, had breakfasted about three hours before, had taken little exercise, and though warm in respect to sensation, they were not disagreeably so, or sensibly perspiring. In each instance the temperature was ascertained, by placing a delicate thermometer under the tongue, near its root, every precaution being taken to insure accuracy. The following were found to be the temperatures of seven different gentlemen:—

[1] The observations of an earlier date than 1820, were first published in the Edinburgh Philosophical Journal for January, 1826.

No.	Age.	Temp.
1	24	99°
2	28	99·5
3	25	98.75
4	17	99°
5	25	99°
6	20	98°
7	28	98.75

On the 21st of March, in latitude N. 0° 12', at noon, when the sun was apparently vertical, the sky clear, a fresh breeze blowing, and the temperature of the air 79·5. I repeated my observations on the same gentlemen, enjoying good health as before, and not unpleasantly warm.

No.	Temp.
1	100°
2	99·5
3	98·5
4	99°
5	99°
6	99·5
7	99°

On the 4th of April, in latitude S. 23° 44', at between twelve o'clock and one in the afternoon, when the weather was very fine, a gentle breeze blowing, and the temperature of the air 80°, I repeated my observations on the preceding gentlemen, and on four more, and on a little girl and a boy. The circumstances were favourable, much the same as those already described, and the individuals not unusually warm, though our sensation of heat was rather more than was agreeable.

No.	Age.	Temp.
1	-	99·5
2	-	99·5
3	-	99·75
4	-	100°
5	-	99·5
6	-	100°
7	-	99·5
8	25	101°
9	40	99·75
10	43	99°
11	40	99·5
12	13	100°
13	4	99·5

Lastly, on the 5th of May, in latitude S. 35° 22, after having been three weeks between this latitude and that of 30°, the weather damp and cool, I repeated my observations on a few of the same gentlemen as before, and at noon when the temperature of the air was 60°, and when we felt cool, almost cold.

No.	Temp.
1	98·5
3	98·25
5	98°
6	98·75
7	98·25
8	98°

I have had an opportunity of observing the effect of the sudden change of temperature on the heat of man in descending from Kandy to Trincomalie.

The town of Kandy, the capital of the interior of Ceylon, is situated in latitude N. 7° 17', and is elevated about 1,500 feet above the level of the sea.[1] Trincomalie is situated in latitude 8° 34.' Kandy is surrounded by hills and mountains, which are covered with wood, and frequently enveloped in clouds, and which abound in springs and torrents. Trincomalie is, at least, fifty miles distant from any mountains. The intervening country is low, and though wooded, very dry, being subject to long-continued drought. One of the consequences of these peculiarities of situation is, that the difference of temperature, between the two places is very considerable. The mean annual temperature of Kandy is about 73·5, whilst that of Trincomalie is about 10 degrees higher; and in the summer and autumn months the difference of temperature is from 12 to 15 degrees.

On the 15th of September, 1818, the day before I left Kandy for Trincomalie, at eight o'clock in the morning, when the air was 69°, I ascertained the temperature, both under the tongue, and in the axilla, of six persons who were to accompany me,—one a servant, the other five, part of a set of palankeen bearers, all natives of the western coast of the island, all in good health, cool, and fasting.

No.	Age.	Temp. under Tongue.	Temp. in Axilla.
1	35	98°	96°
2	20	98°	97°
3	40	99°	97°
4	35	98°	97·5
5	20	98°	97·5
6	24	98°	97°

On the 3d of October, the day after our arrival at Trincomalie, t nine o'clock in the morning, when the temperature of the air was 3°, I repeated my observations on the same men, who had not reakfasted, were in good health, and were not fatigued, having ome the last fourteen miles the day before by water.

[1] This elevation I ascertained by means of the barometer. For the exact latitude of Kandy I was indebted to G. Lusignan, Esq.

No.	Temp. under Tongue.	Temp. in Axilla.
1	99°	97°
2	99°	97·25
3	99°	97°
4	99·75	99°
5	99·5	99°
6	99·5	98°

Again, on the 19th of October, the day before we set out on our return from Kandy, at half-past eleven o'clock in the morning, when the temperature of the air was 82°, I renewed my observations on the men, who, since the 3d of the month, had been leading an idle life at Trincomalie; they had breakfasted about two hours before, and none of them seemed to feel disagreeably warm.

No.	Temp. under Tongue.	Temp. in Axilla.
1	102°	99°
2	101°	98·75
3	98·5	97·5
4	99°	98°
5	99°	98°
6	100°	99°

On the 28th of October, two days after our return to Kandy, at half-past eleven o'clock in the morning, I tried, for the last time, the temperature of these men, with the exception of two who were absent. They were in good health, though hardly recovered from the fatigue of a rapid harassing journey, in cool wet weather, through a country on the eve of breaking out into rebellion, the temperature of the air at that time had suddenly risen to 84° from 69°, which it was at seven A. M.

No.	Temp. under Tongue.	Temp. in Axilla.
1	98·5	99°
2	98°	97°
3	98°	97·75
4	98°	97·5

Kandy, from its peculiar situation, so near the equator, nearly in the middle of a large island, elevated as it is above the level of the sea, and surrounded by mountains, is subject to considerable vicissitudes of temperature in the course of the day, and, consequently, is a place well adapted for making observations on the effects of these vicissitudes on animal heat. In fine weather the temperature of the air, at sun-rise, is always below 70°, and I have seen it as low as 55°; and in the afternoon, in such weather, it is always above 76°, and frequently as high as 83°.

On the 18th of January, 1818, a favourable day, I tried the temperature of an individual, at different hours, and obtained the following results.

Hour.			Temp. of Air.	Temp. under Tongue.	Sensation.
6	A. M.		60·5	98°	Cool.
9			66°	97·5	Cold.
1	P. M.		78°	98·5	Cool.
4			79°	98·5	Warm.
6			71°	99°	Warm.
11			69°	98°	Cool.

It may be proper to mention how the individual passed the day. He rose at six A. M.; read till nine A. M.; breakfasted temperately at ten; was engaged in some chemical experiments from half-past ten to two P. M.; from two P. M. to five was employed chiefly in reading; from five to six took gentle horse exercise; dined sparingly between seven and eight; drank only one glass of wine; and, lastly, from nine to eleven was most of the time employed in writing.

It would be tedious to give other instances illustrative of the change of the temperature of man,—increasing with the temperature of the air, and falling as the atmosphere cools within certain bounds. The preceding instance, which has been confirmed by various experiments I have made, is the most minute and satisfactory that I can bring forward. The subject is inconvenient to make experiments on, and particularly for a person whose time is not his own, and, as a professional man, has seldom a day of leisure, the whole of which he can spend as he chooses. Nor is it easy in this inquiry to arrive at accurate results,—at any thing more than an approximation to the truth,—in consequence of the effects of a number of circumstances, and particularly of health, diet, and exercise, which cannot be duly appreciated until they have been more minutely investigated.

2.—*Of the temperature of different races of men.*

At the Cape of Good Hope, at the Isle of France, and in Ceylon, I have had opportunities of trying the temperature of several different races of men.

At the Cape, in the winter of 1816, on the 24th of May, at noon, when the temperature of the air was about 60°, I prevailed, with some difficulty, on five Hottentots, to allow me to put a thermometer into their mouths, for they were afraid of the instrument; and, when I saw them again, one blamed it for an illness with which he was seized soon after submitting to the experiment. I found their temperatures the following:—

No.	Temp. under Tongue.
1	98°
2	96·5
3	96·5
4	97·75
5	99·5

These Hottentots, I may remark, were in the service of our government, employed as artillery drivers. They were in good health, and resting at their barrack at the time. Their ages varied from twenty-five to forty, judging from their looks, for of the years they had lived, they themselves had taken no account. Those whose temperatures were lowest, I may mention, were most meagre and wretched in appearance ; and, indeed, with the exception of No. 5, who was pretty robust, they were all of a very spare habit of body.

At the same time I ascertained the temperature under the tongue of three English artillerymen, in good health and cool, who had served ten years at the Cape, and who were giants in appearance when compared with the poor Hottentots. All three were nearly of the same age, between thirty and forty. The temperature of one of them was 99·5, of the other two 99°.

At the Isle of France, at Port Louis, in June, one of the coolest months there, when the thermometer was at 74°, I tried the temperature of three negroes, two natives of Madagascar, and one of Mozambique. Of these the temperature of the two first was 98°, and of the last 99°. They were house slaves, between eighteen and twenty years old, well clothed and fed, and in good health.

The temperature of an English gentleman, who had been in the island several years, and of another just arrived, was ascertained at the same time. The former was 98·25, the latter 98·5.

In the island of Ceylon I commenced my observations, at Colombo. Colombo, in latitude N. 6° 56', is situated on the sea-shore, at the distance of about thirty miles from the boundary mountains of the Kandian country. Its temperature is remarkably equal ; in the hottest day seldom exceeding 84°, and in the coolest night rarely falling to 70°. The greater part of the year, the range of the thermometer is from 77° to 73°, and the mean annual temperature is about 79°.

On the 14th of September, between six and seven o'clock in the morning, in a village about a mile from Colombo, when the air was about 79°, I tried the temperature of six Singalese, of different sexes and ages, all cool and in good health and fasting.

No.	Sex.	Age.	Temp. under Tongue.
1	F	50	101°
2	-	4	101·5
3	M	29	101°
4	-	8	101·5
5	-	40	100°
6	-	25	100°

These people lived in the midst of a cocoa-nut grove, and, like the Singalese in general, led an easy and indolent life, according to our notions of activity, and subsisted chiefly on rice, fruit, and vegetables.

The following morning, about the same time, and when the tem-

perature of the air was the same, I tried the temperature of four
Albinoes.

No.	Sex.	Age.	Temp. under Tongue.
1	F	5	101·5
2	-	12	101·5
3	-	23	101·75
4	M	27	101°

These Albinoes were the children of black parents; the two first
were sisters, and they had brothers and sisters of the colour of their
parents.   They were all well made, active, and in good health.[1]

On the 12th of October, between six and seven o'clock in the
morning, while the air was between 77° and 79°, I tried the tem-
perature of a number of children, at the orphan school, in the
neighborhood of Colombo, some half caste, of Singalese mothers
by English soldiers, others white of English parents.

HALF   CASTE.

No.	Sex.	Age.	Temp under Tongue.	Temp. in Axilla.
1	F	12	100·5	98·5
2	-	14	101°	
3	-	17	100°	
4	M	14	102°	100°
5	-	10	101·5	99·5
6	-	14	100°	99°
7	-	10	100°	99°

WHITE CHILDREN.

1	F	9	101°	99·5
2	-	6	101°	98°
3	-	9	101°	98·5
4	-	12	102°	100°
5	M	8	102°	100°

These girls and boys at the excellent institutions to which they
belonged, were well clothed and fed, as well as usefully educated.
At the time, they were cool, and in good health, and had not break-
fasted.

In the Kandian country, the climate of which in general very
much resembles that of its capital, I have at different times ascer-

[1] The young Albino, twelve years of age, in England. and certainly in
Norway, would not be considered peculiar; for her eyes were light blue, and
not particularly weak, her hair of the colour that usually accompanies such
eyes, and her complexion fresh and rather rosy.   She had considerable pre-
tensions to beauty, and was not without admirers amongst her countrymen.
It is easy to conceive that an accidental variety of the kind might propagate.
and that the white race of mankind is sprung from such an accidental variety.
The Hindus are of this opinion, and there is a tradition or story amongst
them in which this origin is assigned us.

tained the temperature of Kandians, Vaidas, Caffres, Malays, Sepoys, and Englishmen.

In Saffragam, a Kandian province, on the 17th of April, 1817, when the temperature of the air was 72° at seven o'clock in the morning, I tried the temperature of an old Kandian, almost a century old, and of a boy about twelve years old, both cool, but not cold:

Old man. Temp. under tongue 95°. In axilla 93°.
Boy. Temp. under tongue 98°. In axilla 96·5.

In Dombera, another Kandian province, on the 5th of September, at one o'clock in the afternoon, when the temperature of the air was 76°, I tried the temperature of three Kandians, stout men in the prime of life.

No.	Age.	Temp. under Tongue.	Temp. in Axilla.
1	24	99°	98°
2	30	99°	98°
3	33	98°	97·5

On the 7th of the same month, and in the same mountainous district, I tried the temrerature of three Kandian priests:

No.	Age.	Temp. under Tongue.
1	15	99°
2	16	99°
3	30	98°

At Kandy on the 7th of February, 1818, I tried the temperature of two young priests at five o'clock in the evening, when the air was 75°.

No.	Age.	Temp. under Tongue.
1	15	99°
2	16	98·5

In the same year, at Kandy, on the 29th of October, I found the temperature of a very old priest, (about a century old) in the axilla 98·5.[1]

The higher castes of Kandians, I may remark, to which the few subjects of my experiments belonged, are, for Indians, not only well formed, but stout and muscular men. Their food consists chiefly of rice and farinaceous fruits, which they use highly seasoned, and of milk, fowls and game. Their drink is principally water, the use of intoxicating liquors being contrary to their religion. Their ordinary dress is a handkerchief about the head, and a large cloth folded about the loins, and reaching below the knee, with the addi-

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[1] Advanced old age in this person was well marked. He complained of all his senses having become impaired, excepting that of taste; he preferred animal food to vegetable, and the most savory meats, and ate often. He said he slept little, and dreamed much; and that his dreams were about the dead.

tion, in cold weather, of another cloth which is thrown over the
shoulders, and wrapped about the body. Their dwellings are
comfortable cottages. Their occupations chiefly agricultural pur-
suits. As they are stouter than the lowlanders, so are they more
active, and as it appears to me, more acute and intelligent.

All the priests whose temperatures I tried, I should observe, were
priests of Boodho, who dress and live in a manner peculiar to
themselves. Their dress consists of yellow robes, which, thrown
over the left shoulder, and girded about the loins, fall in graceful
drapery to the feet, covering every part with the exception of the
neck, right arm and shoulder. They wear nothing on their head,
which, as well as the eye-brows and the hairy part of the face, is
carefully shaved and kept bare. They profess celibacy, lead an indo-
lent, quiet life, devoted chiefly to religious duties and literary pursuits,
(such as they are) and subsist almost entirely on vegetable food.

At Kandy on the 12th of September, 1818, I had an opportunity
which rarely occurs, of ascertaining the temperature of three
Vaidas. The temperature of the air at the time was about 78°.

No.	Age.	Temp. under Tongue.	Temp. in Axilla.
1	60	98°	95°
2	30	98°	96°
3	35	98·5	96°

The ages of these men I was obliged to guess, for they themselves
could not inform me. They belonged to a large party who had
come to Kandy, with a tribute of dried deer's flesh and wild honey.
They were quite naked with the exception of the *partes naturales*,
which were concealed by a scrap of cloth. The hair of their head
and beard was long and matted, and had never been cut or combed.
Their eyes were lively, wild and restless. They were well made
and muscular, but of a spare habit, and in person they chiefly dif-
fered from the Kandians in the slightness of their limbs, the wild-
ness of their looks, and their savage appearance. According to
their own account of themselves, they came from the neighbourhood
of the Lake of Bintenne, where they subsisted on game which
they killed in the chase, some roots, and wild fruits, and a little
grain of their own growing. They were profoundly ignorant,
could not count above five, were hardly acquainted with the
rudiments of any art, and though they feared demons as they did
wild beasts, they had no knowledge whatever of a Supreme benefi-
cent Being, and not the slightest notion of any state of existence
after the present. Yet, these men considered themselves civilized
in comparison with the wilder tribes of Vaidas, who never leave
their native forests, and who attack with their sylvan weapons, the
bow and arrow, every intruder into their haunts, and whom I have
heard Kandians of a bordering province describe as living almost
entirely on raw animal food, as going quite naked, as having no

superstition, and in fact as being in a state very little removed from that of brutes.[1]

On the 17th of December, 1818, when the air was 74°, I tried the temperature of five African negroes, servants in the military hospital at Kandy.

No.	Age.	Temp. under Tongue.	Temp. in Axilla.
1	23	98·5	98°
2	35	98·5	98°
3	25	99°	98°
4	34	99·5	98°
5	28	99·5	98°

The ages of these men I conjectured from their looks.   Most of them were from Goa.   They were of African parents, had not degenerated, and like African negroes in general, they were stout and muscular.   Nos. 4 and 5, I should remark, whose temperatures exceeded the rest, were in a state of gentle perspiration produced by slight exercise.

On the 18th of March, 1818, at noon, air 81°, at Kandy, I tried the temperature of four Malays :—

No.	Age.	Temp. under Tongue.	Temp. in Axilla.
1	17	98·5	98·5
2	35	99·5	97·5
3	22	·99°	98°
4	18	98·5	97·5

These men were free Malays, in good circumstances.   Three were natives of Colombo, and one of Cochin.   They were active, stout, well made, and very muscular men, all of Javanese parents. They were dressed not unlike Kandians, but with less cloth about their loins, and a cloth most commonly over their shoulders.

On the 18th of May, in the afternoon, when the temperature of the air at Kandy was 80°, I tried the temperature of six Sepoys, belonging to a battalion of Madras native infantry :

No.	Age.	Temp. under Tongue.	Temp. in Axilla.
1	25	98.5	98°
2	19	99°	96°
3	26	98·5	97°
4	22	98°	95°
5	38	100°	97°
6	20	98°	97°

[1] The influence of habitual exercise in strengthening any particular set of muscles, is remarkably illustrated in the Vaida.   I saw one, a young man of a diminutive and spare form, with slender arms and shoulders, use with the greatest ease a bow he had been accustomed to, which one of the strongest of our soldiers could hardly bend.

Most of these Sepoys were natives of Madras, or of the adjoining country. They were tall, thin, and rather feeble men. They had been in Ceylon about three months.

On the 20th of the same month at between eight and nine o'clock in the morning, when the temperature of the air in Kandy was about 75°, I tried the temperature of several English soldiers.

No.	Age.	Years in India.	Temp. under Tongue.
1	24	0½	98·75
2	99	2	98·5
3	27	2½	99°
4	36	16	99·25
5	28	4	99°
6	34	0½	99·5
7	25	1	100°
8	23	0½	101°
9	25	25	99°
10	23	0½	98°

The four first were in perfect health ; the remaining six were in different stages of convalescence from intermittent fever. They were all cool, and had not breakfasted.

3.—Of the temperature of different kinds of Animals.

My observations on the temperature of different kinds of animals have been made at intervals, as leisure and opportunity permitted, in England, Ceylon, and during a voyage to the latter island. Though pretty numerous, they are far from complete ; the completion of the inquiry demands the co-operation of many observers.

1st, Of the temperature of the Mammalia.

I may premise that in my experiments on the mammalia, with a few exceptions which will be particularly noticed, the temperature of each animal was ascertained by introducing a thermometer into the *rectum ;* and I may extend the remark to the experiments on birds ; and I may further premise, that, when the contrary is not noticed, the subject of the experiments appeared to be healthy.

Monkey (Simia Aygula).—At Colombo, on the 30th of May, air 86°, the temperature of this animal full grown, in the axilla was 104½°; rectum, only 103½°.

At Amanapoora, in the Kandian country, on the 1st of June, air 73°, the temperature of another full grown monkey of the same kind, in the axilla, was 101.°

Pangolin (Manis pentadactyla).—At Colombo on the 4th of November, air 80°, the temperature of a young pangolin apparently sickly, was only 90°.

Bat.—In the neighbourhood of Colombo, on the 27th of September, air 82°, the temperature of one bat was 100°, and that of another 101°. The instant the animals were killed, the thermometer was introduced into the cavity of the abdomen. The species resemble the *Vespertilio peruviana* of Linnæus, but was much smaller.

V. vampirus.—At Colombo, on the 15th of October, air 70°, the temperature of this animal, ascertained in the same way as the preceding, was 100°.

Squirrel (Sciurus getulus ?)—At Colombo, on the 19th of October, air 81°, the temperature of this animal was 102°.

At the same place, on the 29th of September, air 84°, the temperature of a large black squirrel was 106°, in the thick fur of the groin.

Common rat.—At Colombo, on the 8th of February, air 80°, the temperature of this animal was 102°.

Guinea-pig.—Female half grown, 102°; male of about the same age 101°.[1] Chatham, 5th May.

Common hare.—At Colombo, on 16th of June, air 80°, the temperature of this animal in the groin was 100°.

Ichneumon.—At Colombo, on the 4th of November, air 81°, the temperature of this animal was 103°.

Jungle-cat—At Colombo, on the 26th of February, air 80°, the temperature of a young animal of this species of Vivera was 99°.

Cur-dog.—At Kandy, on the 29th of May, the temperature of an animal of this kind was 102·5, and of another 103·5, both nearly full grown. The temperature of a bull-dog, in England, on the 27th December, was 102°.

Jackall.—At Colombo, on the 9th of April, air 84°, the temperature of two young jackalls was 101°.

Common cat.—In London, on the 5th September, air 60°, the temperature of a full grown cat was 101°, and in Kandy, on the 7th April, air 79°, the temperature of another was 102°.

Felix pardus.—At Colombo, on the 10th of February, air 81°, the temperature of a young fierce animal of this kind, about four months old, was 102°.

Horse.—At Kandy, on the 14th of June, air 80°, the temperature of a horse, of Arab descent, was 99·5.

Sheep.—In Scotland, I have observed the temperature of sheep in summer, to vary from 101° to 104°; at the Cape of Good Hope, in winter, air 67°, in six different instances I found the temperature of the African sheep to vary from 103° to 104°, and in Ceylon, in the neighbourhood of Colombo, air 78°, the temperature of one sheep was 104°, and that of another 105°.

Goat.—At Mount Lavinia, near Colombo, on the 27th December, air 78°, the temperature of a full grown castrated goat was 103°, that of a female, about nine months old, 104°.

Ox.—At Edinburgh, in summer, the blood of an ox, flowing

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[1] In abdomen, under liver, 103°.

from the carotids, was 100°; in Kandy, on the 28th of May, air 80°, the temperature of an ox, ascertained in the same way, was 102°.

Elk.—At Mount Lavinia, on the 27th of December, air 78°, the temperature of a female elk was 103°.

Hog.—At Hanwille, in Doombera, on the 26th of November, air 75°, the temperature of the blood of a wild hog, flowing from the carotids, was 105°; at Mount Lavinia, air 80°, the temperature of two young domestic pigs was the same.

Elephant.—At Colombo, on the 22d of September, air 80°, the temperature of a full grown, healthy elephant was 99·5.   It was ascertained by placing a thermometer in a deep abscess in the back.[1]

Porpoise.—In latitude N. 8° 23', on the 11th of March, air 72°, sea 74·75, the temperature of a porpoise was 100°.   The animal was drawn into the ship alive.   The instant it was killed I tried its temperature, by introducing a thermometer into the substance of its liver.

2.—*Of the temperature of birds.*

Falcon (Falco milvus?).—At Colombo, on the 24th of August, air 77·5, the temperature of this bird was 99°; it had been shot a few hours, and its legs were broken.

Screech-owl.—In London, in the autumn, air 60°, the temperature of this bird was 106°.

Jackdaw.—At Attapittia, in the Kandian country, on the 2d of June, air 85°, the temperature of this bird, the instant it was shot, was 107·75.

Common thrush.—In London, in the autumn, air 60°, the temperature of this bird was 109°.

Common sparrow.—At Gompala, in the Kandian country, on the 3d of June, air 80°, the temperature of this bird, the instant it was shot, was 108°.

Common pigeon.—In London, in the autumn, air 60°, the temperature of this bird, confined in a cage, was 108°.   At Mount Lavinia, on the 27th of December, air 78°, the temperature of two young pigeons, two weeks old, was 109·5 ; and of two, three weeks old, 109°.

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[1] It was necessary to lay open the abscess to effect a cure. The operation was performed with an amputating knife ; a deep incision, as was requisite, was made ; the animal was kneeling down for the convenience of the operator—not tied—his keeper at his head.   He did not flinch, but rather inclined towards the surgeon, uttering a low suppressed groan.   He seemed conscious, that what was doing was intended for his good ; no human being, similarly situated, could have behaved better.   I think it right thus to record this instance, which I witnessed myself, of this animal's (may I call it) reflecting power and conduct, which it is difficult to consider otherwise than rational.   And so confident were the natives that he would behave as he did, that they never thought of tying him.

Jungle-fowl.—In Ceylon, near Tangalle, on the 20th of July, air 78°, the temperature of one jungle-hen, the instant it was shot, was 107·5; and in the afternoon of the same day, air 83°, the temperature of another was 108·5. The jungle-fowl of Ceylon, I may remark, more resembles the English pheasant than the barndoor fowl.

Common fowl.—At Edinburgh, in the winter, air 40°, the temperature of a full grown hen was 108·5. At Mount Lavinia, in December, air 78°, the temperature of two hens was 110° (the one, half, the other full grown); that of a hen that had been sitting on her eggs three weeks 108°; that of an old cock 110°, and of two chickens, two months old, was 111°.

Guinea-fowl.—At Mount Lavinia, at the same time, the temperature of a full grown guinea-hen was 110°.

Turkey.—At the same time, the temperature of a full grown turkey-cock was 109°, that of two more of the same age 108·5; that of a full grown hen 108°; and that of a young cock two months old, was 109·5.

Procellari æquinoctialis.—In latitude N. 2° 3', on the 8th of August, air 79°, sea 81·5, the temperature of this bird was 103·5, and that of another 105·5.

P. Capensis.—In latitude S. 34° 1', on the 11th of May, air 59°, sea 60°, the temperature of two birds of this kind was 105·5.

At Mount Lavinia, on the 27th December, when the temperature of the air was about 77°, the following observations were made on some of the inmates of the governor's poultry-court.

Hen, half grown	110°
Cock, ditto	111°
Chicken, two months old	111°
Another, of the same age	111°
Hen, full grown, laying eggs	110°
Hen, that had been sitting on her eggs about three weeks	108°
An old Malay cock	110°
A cock, ten weeks old, sickly	108°
Guinea-fowl, full grown	110°
Turkey-cock, full grown	108°
Another, do.	108°
Another, do.	109°
Turkey-hen	108°
A young turkey, two months old	109·5
A full grown goose	107°
Another	107°
A goose, that had been sitting on her eggs nearly a month	106°
A young duck, twenty days old	110°
Another, five weeks old	110°
Another, two months old	110°
Another, of the same age	110°

Two full grown ducks   -   -   110°
A full grown drake   -   -   110°
Another, but younger   -   111°
A full grown teal, taken young, tamed   -   109·5
Another   -   -   -   108°

   Young snipe.—Colombo, Ceylon, September, air 83°, half
fledged; thermometer, between thigh and body, 98°.

   Another, less strong, 97°. It ate a few worms, seemed distressed,
and lived only about 24 hours after.

   Common hen.—Colombo, November, *in recto*, 109°; after having
been frightened by a cobra capello, not bitten, 111°.

   At Trincomalie, in the same year, on the 9th of October, when
the temperature of the air was 84°, that of five fowls, nearly full
grown, was 107°. They had been in confinement in a small
hamper two or three days. On the 18th October, in the same place,
when the air was 82°, found the temperature of two full grown
hens, not in confinement, also 107°.

   Plover.—Near Minery, in Ceylon, in September, the temperature
of a plover, just killed, was 105°.

   Common goose.—At Mount Lavinia, in December, air 78°, the
temperature of two full grown geese was 107.

   Common duck.—At the same time and place, the temperature
of a full grown drake, of two full grown ducks, and of four duck-
lings, from three to five weeks old, was 110°, and that of a young
drake, full grown 111°.

   Peacock.—At Kornegalle, in July, air 83°, the temperature of a
full grown wild peacock was 105°; of a young pea-hen 108°.

Of the temperature of the amphibia.

   Testudo mydas.—In latitude N. 2° 27′, on the 19th of March,
air 79·5, the temperature of a large turtle, caught a week before at
Ascension, was 84°, *in recto*. Again, in latitude S. 2° 29′, on the
23d of March, air 80°, the temperature of the blood of the animal
flowing from the great vessels of the neck was 88·5. The turtle
was sickly, and probably this heat was morbid. At Colombo, on
the 4th of May, air 86°, the temperature of the blood of a turtle
that had been caught the day before was 85°.

   T. geometrica.—At Cape Town, in May, air 61°, the tempera-
ture of this animal was 62·5. At Colombo, on the 3d of March,
the temperature of a larger specimen was 87°, air 80°.

   Rana ventricosa.—At Kandy, on the 31st of May, air 80°, the
temperature of two frogs of this kind, just brought from a damp
shaded place, was 77°.

   Common male frog, in recto, 64°, immersed in water of 60°, in
a small pond, near Edinburgh, June 23, air 60°.

   Another frog, kept in shade out of water a quarter of an hour in
recto, 63°, air 62°. A decapitated frog of about the same size, kept
there the same time, 62°, in recto.

A frog, just after it was taken, (it had hopped some way) after basking on wet turf, exposed to the sun, occasionally obscured by light clouds, in recto, 70°; water of pond 61°. The same frog placed in the shade, where the thermometer was 62°, after a quarter of an hour, from 70° fell to 63°; and decapitated, and left another quarter of an hour, the air continuing at 62°, it fell to 60°, in recto.

Another frog, in a state of rest, taken from a shady place, surrounded with damp plants, in recto, 58°; the thermometer in the place from whence it was taken was also 58°.

Another frog, taken from the turf close to the pond, where it had been a short time basking, was 68°, *in recto ;* the thermometer exposed in the same place amongst the grass was 65°.[1]

Iguana.—At Colombo, 4th September, air 82°, the temperature of this animal was 82·5.

Serpents.—At Colombo, on the 27th of August, air 81·5, the temperature of an elegant green snake, a species of coluber, was 88·5, *in œsophago.* At the same place, on the 24th of August, air 82·5, the temperature of a small species of brown snake, another species of coluber, was 81·5 *in abdomine.* On the 23d of September, air 83°, the temperature of different species of brown snakes also belonging to the genus coluber was 90°, *in œsophago.*

4.— *Of the temperature of fishes.*

Shark.—In latitude N. 8° 23', on the 11th of March, 1816, air 71·75, sea 74·75, the temperature of a large female shark, just taken, and still alive, was 77° in the deep muscles near the tail.

Bonito.—In latitude S. 1° 14', on the 29th of July, 1816, air 78°, sea 80·5, the temperature of the heart of this fish, which lies very near the surface, was 82°, and of the deep-seated muscles 99°. These observations were made immediately after the fish was taken. I may remark, that the heart and gills of this fish were unusually large, and the latter of a dark red colour ; further, that the muscles in general, which were very thick and powerful, were red like those of a porpoise, and that the bonito appears to be as fond of raising its head above the water as the porpoise itself: with these circumstances, probably its extraordinary temperature is connected.

Common trout.—Near Edinburgh, in the spring, river 56°, the temperature of this fish was 58°.

Common trout. Mount Cenis,[2] in a tank supplied with water flowing perennially from snow, I found at 40°, at three different seasons of the year, temperature of fish 42°, in excellent condition.

[1] These frogs were all males (no females were found in the pond), they were full grown, and active ; their testes large. In their stomachs were caterpillars and insects, and in one, two small hard stones.

My observations seemed to show, that their temperature is variable, as might be expected, according to the activity of their respiration ; and that, occasionally, it is at least three degrees higher than the moist body would be similarly circumstanced, independent of respiration.

[2] The trout at Mount Cenis, in a lake about 5000 feet above the level of

Trout, about half a pound weight, *in œsophago*, 58°; water of river, flowing from Loch Katrine into Loch Vanacher, 56°, July 5th.

Eel (*M. laterostris*), Chatham, May 14. Temperature in air 51°, *in œsophago*, 51°. The eel had been out of water several hours.

Flying fish.—In latitude N. 6° 57', on the 12th of March, air 77°, sea 77·5, the temperature of this fish, the instant it fell on the deck, was 78°.

5.—*Of the temperature of mollusca.*

Common oyster.—On a rock about a quarter of a mile from the shore, off Mount Lavinia, where the water was about a foot deep, in December, the temperature of the common oyster was the same as that of the sea, viz. 82°.

Snail.—At Kandy, on the 11th of June, the temperature of one of a large species of snail that abounds in the woods of Ceylon, was 76°, and that of another, 76½°, after having been confined eight hours in a box, the temperature of which was 76¼°.

6.—*Of the temperature of crustacea.*

Cray-fish.—At Colombo, on the 16th of September, air 80°, the temperature of a large cray-fish that had been taken out of the sea two or three hours before, was 79°.

Crab.—In the neighbourhood of Kandy, on the 25th of March, the temperature of a small crab, of a species which is common in the mountain torrents of the interior, was the same as that of the water in which it lived, viz. 72°.

7.—*Of the temperature of insects.*

Scarabæus pilularius.—At Kandy, on the 30th of June, air 76°, the temperature of a beetle of this kind was 77°.

Glow-worm.—At Kandy, on the same day, in the morning, air 73°, the temperature of a large species of glow-worm was 74°.[1]

the sea, is always in season, which perhaps may be owing in part to the temperature of the deep water being probably always about 40°. In accordance with this, I am informed that the same is the case with the char inhabiting the deep waters of Coniston Lake, in Lancashire, and also it is said, of the trout of the Sorgue, at Vaucluse, in Provence—a stream which bursts out at the base of a mountain precipice, and the temperature of which probably varies very little throughout the year. On the 10th of April, 1830, I found it 54° at its source.

[1] The light of the glowworm does not appear to be combined with heat; and the same remark applies to the light of the fire-fly, and, I believe of other luminous animals. On the luminous matter of the fire-fly, I made many experiments in 1816, at Colombo. I may mention here a few of the results. It is viscid, neither acid nor alkaline, has a strong and peculiar

Blatta orientalis.—At Kandy, on the 28th of the same month, air 83°, the temperature of two cockroaches was 75°; and on the 29th, found the temperature of two more the same, when the air was 74°.

Gryllus hæmatopus ?—At the Cape of Good Hope, in May, air 62°, the temperature of two locusts was 72·5.

Apis ichneumonia ?—At Kandy, on the 26th of June, air 75°, the temperature of a wasp was 75°.

Papillio Agamemnon.—At Kandy, on the 2d July, air 78°, the temperature of this butterfly, in abdomine, 80°, and in thorace, 81·5.

A large butterfly not unlike the preceding, in England, Nov. 4th, temperature of room 74°, thermometer placed between its wings and abdomen rose to 81°.

Scorpio afer.—At Kandy, on the 20th May, at noon, air 79°, the temperature of a large scorpion was 77·5.[1]

Julus.—At Kandy, on the 18th of June, at noon, air 80°, the

odour; affords no ammonia when decomposed by heat; it yields a brown empyreumatic oil. It continues to shine after it has been detached from the animal; in one instance, it shone after fourteen hours. The separation of the luminous part does not immediately prove fatal to the fly; I have known one live twelve, and even eighteen hours after. The luminous matter appears to be a secretion independent of the influence of external light: I kept the flies in darkness many days, supplying them with food, and they continued to shine. The life of the animal is more easily extinguished than its luminous property; thus, when plunged into carbonic acid, the luminous matter continued shining, after the animal was motionless; but its duration was short: the same was observed in hydrogen gas. The luminous matter detached, shines for a brief time in water, and even in water deprived of air, by long boiling, in alcohol, in aqua ammoniæ, nitric acid, nitrous acid vapour, ammoniacal vapour, in carbonic acid, and in hydrogen. In the nitric acid, and the aqua ammoniæ, and their vapours, and in alcohol, the brightness of the light was increased on immersion; but it was of only a few seconds' duration. In carbonic acid and hydrogen, it shone longer isolated than when forming a part of the insect. Sixteen fire-flies, confined in a small vial afforded sufficient light to enable me with perfect ease to see the figures on my watch. The gland or organ by which the luminous matter is secreted, has a tubular arrangement, somewhat perhaps analogous to the electrical organs of the gymnotus and torpedo.

[1] The popular idea of the poison of the scorpion, is, I believe, far from correct. From the few experiments I have made with this animal, I am induced to infer that its sting is less severe than that of our honey bee. A fowl wounded by a large scorpion in the breast, appeared to suffer little or no inconvenience; the punctured part was only very slightly red, and just perceptibly swollen. I have applied the peculiar fluid twice to the tongue; in one instance, it occasioned a slightly acrid sensation; and in the other, besides the sensation, it blistered the part slightly; the effect was very transitory; in a few hours it had ceased.

Even in Ceylon, the poetical idea, of the enraged scorpion stinging himself to death, is entertained; but I never witnessed such a result. On one occasion I saw the experiment of encircling a scorpion by fire, confidently made by an English gentleman, but without effect, no danger from the flames, no provocation could induce the animal to wound himself. Perhaps, occasionally, when extremely irritated, it may do so,—not knowing what he is doing.

temperature of a julus was 78.5. It was of that species which emits a yellowish fluid, having the smell of iodine, and which colours the cuticle, not unlike iodine, but has no effect on polished steel.

8.— *Of the temperature of worms.*

The only worms, the temperature of which I have tried, were two kinds of leech, the *Hirudo sanguisuga*, and a species which in Ceylon is called the jungle leech, remarkable for living out of water in damp places. The temperature of both was the same as that of the water and air in which they were confined.

I may remark, generally, that in the few experiments I have made to ascertain the temperature of small animals of the lower classes, a very small thermometer was used in each instance, introduced through a small incision into the body.

IV.— *Conclusions and general remarks.*

That the temperature of man increases in passing from a cold or even temperate climate, into one that is warm ; that the temperature of the inhabitants of warm climates is permanently higher than those of mild; and that the temperature of different races of mankind, *cæteris paribus*, is very much alike, are conclusions which the preceding observations on man seem to warrant.

The first conclusion, I am aware, is not novel ; but I do not know that it was ever drawn before, excepting from very scanty data.

The second conclusion, though conformable with the first, is I believe new ; indeed it is contrary to a received opinion, that the temperature of man in warm climates is actually lower than in cold. The opinion alluded to, I conceive, arose partly from hypothetical views of the subject ; and if I recollect rightly, it has been supported only by two or three observations recorded by Dr. Chalmers, in his History of South Carolina, which were made at a time when thermometrical experiments were not very common, and when the standard temperature of man was rated much too low. Further refutation of this opinion is perhaps unnecessary. The experiments I have made with all the care in my power, are so numerous, and their results are so consistent, that if I do not deceive myself, they put the question beyond the shadow of doubt, and fix as a fact, that if the standard temperature of man, in a temperate climate, be about 98°(which I believe is the greatest approximation to the truth) in a hot climate it will be higher, varying with atmospheric variations from $98\frac{1}{2}°$ to 101°.

The third conclusion I believe to be perfectly accurate ; I say *believe*, because it is difficult, if not impossible, to collect more than presumptive evidence on the subject. However, may not the evidence be considered sufficiently satisfactory, since the variation

of the temperature of the different races I tried did not exceed, in degree, what may be witnessed amongst different individuals of a ship's company, all of one nation, or even amongst different members of the same family? The similarity of temperature in different races of men is the more remarkable, since between several of them whose temperatures agreed, there was nothing in common but the air they breathed,—some feeding on animal food almost entirely, as the Vaida,—others chiefly on vegetable diet, as the priests of Boodho, —and others, as Europeans and Africans, on neither exclusively, but on a mixture of both.

Farther, that the temperature of birds, of all animals is the highest,—that of the mammalia next,—that of the amphibia, fishes, and certain insects next in degree,—and lowest of all, that of the mollusca, crustacea, and worms,—are conclusions, with few exceptions, that may be deduced from the preceding experiments on the temperature of animals in general.

Moreover, since in general, so far as experiment and observation have yet gone, there appears to be a decided connection between the quantity of oxygen consumed by an animal, and the animal's heat, is there not good reason to consider the two in the relation of cause and effect?

If animal heat be owing to nervous energy, or any way connected with the nervous system, why, it may be asked, are birds so much hotter than the mammalia? Why is the temperature of most quadrupeds higher than that of man?

Or, if it be owing to digestion, and secretion, and animal action, why is the temperature of the amphibia and of fishes so low, whose powers, in respect to these functions, are so considerable?

Or, if it be connected with muscular energy, why are the animals, whose muscular powers are most remarkable (the animals belonging to all the lower classes), equally remarkable for the lowness of their temperature?

Or, lastly, if animal heat at all depend on peculiarities of structure or organization, why, it may be asked, is not the temperature of the amphibia elevated like that of birds,—the structure of the respiratory, and digestive, and secreting organs of the one class being so much like those of the other?

XXVII.

OBSERVATIONS ON THE EFFECT OF VIOLENT EXERCISE ON THE TEMPERATURE OF THE BODY.

Whilst I was in Ceylon, I made a few observations on the effect of severe exercise on the temperature of man, which, although

less perfect and extended than I could wish, may be deserving of insertion here.

On the 5th of July, 1817, preparatory to setting out on a journey, I tried the temperature, both under the tongue, and in the axilla, of six natives belonging to a set of palankeen bearers, in good health and quite cool, at about seven o'clock in the morning, when the temperature of the air was 80°. The results were the following:

No.	Age.	Therm. under Tongue.	Axilla.
1	35	98·5	-
2	30	99°	97°
3	35	98·5	96°
4	32	98·5	96°
5	35	99°	96°
6	33	100°	98°

At the end of the 2d day's journey, at four o'clock in the afternoon, immediately on halting, I tried the temperature of three of the bearers.

No.	Therm. under Tongue.	Axilla.
1	98°	98°
3	100°	98·5
4	98·5	98°

Before setting out, when at rest, the skin of these men was cool, it conveyed, indeed, to the touch of another, the sensation almost of coldness; now, instantly after rather severe exercise, when the temperature under the tongue was not raised, that of the axilla was increased and the surface of the body generally felt hot.

The next trial of temperature, under the influence of exercise, was made on the 26th July, on two of the same set of bearers, both about thirty years of age, both in good health, one stout and robust, the other rather slender. At ten A. M., at Rannè, in the Megampattoo, when the temperature of the air was 87°, after resting two hours, the temperature of the first man was, under the tongue, 99°, in the axilla 97°; and that of the second, under the tongue, was 98°, and in the axilla 97°.

A few minutes after ten o'clock, I set out for Tangalle. The sun was bright, the weather unusually hot, and the men, as if impatient, proceeded with more than ordinary diligence and speed. At noon, when the air was 92°, I suddenly ordered them to stop, that I might ascertain their temperature. The first mentioned man, was one of the bearers carrying the palankeen, the other had been running by its side; both were sweating, the latter most profusely. The temperature of the first, under the tongue, was 98°, in the axilla 98·25; of the second, under the tongue, 99°; in the axilla 96·5.[1]

[1] I shall mention one observation more. On the 10th of September, 1817, after walking four miles over a very rugged road, in the interior of Ceylon,

There are, on record, many instances of sudden death,—from drinking cold water, or from plunging into cold water, after exhausting fatigue, and when the body is commonly said to be heated. Probably, in such cases, in conformity with the above observations, the temperature of the body has been actually reduced below its natural standard, taking the tongue as an index of the internal heat, and the fatal effect may, in part, be the consequence. This view of the effect was taken by the late Dr. Currie, who, in his Medical Reports, in the twelfth chapter, has collected many examples of the kind, and, amongst them, that interesting and impressive one of the catastrophe which befell the army of Alexander the Great, on the banks of the river Oxus, where, according to Quintus Curtius, the loss of life was actually greater than had been experienced in any single battle. The circumstances of the case were, a forced march of forty-six miles, in hot weather, over a desert ; excessive thirst and exhaustion ; and, in this state, drinking large draughts of cold water.

XXVIII.

OBSERVATIONS ON THE TEMPERATURE OF THE INSANE.

In the Lunatic Asylum at Fort Clarence, attached to the General Military Hospital at Fort Pitt, Chatham, it has commonly been believed that the temperature of the insane is below the average temperature of man. This opinion has chiefly been founded on thermometrical observations, made many years ago by the late Mr. Tully, then surgeon to the forces, in medical charge. As the difference of temperature was, in many instances, stated to be of several degrees, varying from 92° to 97°, as ascertained under the tongue, taking 98° as the standard, I could not avoid entertaining doubts respecting the accuracy of the results ; and this feeling induced me to institute some trials, for the purpose of endeavouring to arrive at some certainty, on a point not without interest, and importance, both theoretically, and in regard to the management of the insane. I shall give two sets of observations ;—one set made in the depth of winter,—the other in the height of summer,—under circumstances, otherwise, as nearly like as possible ;—viz. at the same

and sweating profusely ; when the sensation of heat was greatest, a thermometer, placed under my tongue, was 98°, the temperature of the air at the time was 88°.

These observations are too few in number to admit of conclusions being drawn from them in a satisfactory manner. Few, however, as they are, the results are not uninteresting, especially those indicating a reduction of temperature under the tongue.

time of the day, between 10 o'clock and noon, at about the same
interval between meals, the breakfast hour being 8, and the dinner
1, and after taking on each occasion about the same degree of
exercise in the open air. The winter observations were made on
the 17th January, 1838, when the temperature of the atmosphere
was 30°; the summer observations, on the 4th of August following,
when it was 68°; in the former instance the temperature of the
room where the trials were instituted, was 43°; in the latter about
the same as the open air.

*Table of observations on the temperature of the insane in the
winter and summer of 1838.*

Name of Patient.	Age.	Species of Insanity.	Winter. State of health	Temp. under T.	Summer. State of health.	Temp. under T.
Pte Henry Dalton	55	Amentia	Pretty good	100	Pretty good	101
— Owen Eagar	63	Mania	good	100	good	101·5
Sjt. George Bayley	49	Mania	good	100	good	101
— H. Sutherland	31	Amentia	rather feeble	99	feeble	101
— John Roarke	41	Mania	good	98	good	101
Pte. Sam. Palmer	42	Amentia	good	99	good	100·5
— John Sharpe	50	Amentia	slightly ailing	102	good	99·5
— William Cane	62	Amentia	good	98	good	100·5
— Hugh Catlin	69	Amentia	feeble	100	feeble	101
— Ad.Walshatsky	51	Amentia	pretty good	99	moderate	100
— William Lyons	31	Amentia	pretty good	100	moderate	101
— Jacob Mundy	53	Amentia	good	99	good	101
— James Mullins	37	Mania	good	99	good	100
— C. T. Levis	41	Mania		101	obscure dis. of lungs	104·5
— James Davis	41	Amentia	pretty good	99	good	100
— George Roper	64	Mania	good	101	good	101
— John Fielding	45	Amentia	pretty good	100·5	moderate	101
— Luke Reilly	29	Amentia	indifferent	99	indifferent	99
— John Hennessy	57	Mania	good	99	good	100
— Michael Tudor	61	melanch'lia	good	100·5	good	101
— John Mayow	40	Amentia	good	100	good	101
— Ricd. Clements	61	Amentia	good	98	good	100
— Christ. Frisch	47	Amentia	good	101	good	101
— Elijah Walwark	27	Amentia	good	98	good	100

None of these patients were under coercion; the majority of them
were chronic cases and incurable, labouring under erroneous ideas
on certain subjects.

For the sake of comparison, I ascertained at the same time, both
in winter and summer, the temperature of some of the individuals
belonging to the establishment.

	Age.	State of health.	Temp. under tongue, 17 Jan.	Temp. 4th Aug.
1 Officer	48	good	98	99
2 Officer	50	delicate	100 5	—
3 Serjeant	53	good	100·5	100·5
4 Serjeant	51	rather infirm	101	101·5
5 Private	41	good	99	99
6 Private	27	good	98	—

From these observations it would appear that the temperature of the insane, as of persons not so afflicted, varies slightly in different individuals; that it is not lower in the insane, but rather higher; and that it varies slightly in summer and winter, being greatest in summer,—in accordance with the preceding observations, made on occasions of rapid transition from one climate to another.

The tolerance of cold and of heat by the insane, is a question totally different from the foregoing on their internal temperature. That they bear in most instances degrees of heat and cold, without complaining, which to sane persons would be disagreeable, is a well-established fact, however it may be accounted for. Probably neither heat nor cold affects their senses in the same degree as it does those in perfect health. Agreeably to this view, it may be remarked, that certain organic lesions occur in the insane, unaccompanied by the ordinary symptoms: thus, tubercles not only form in their lungs, but also tubercular excavations, often unattended with cough or difficulty of breathing, as if the parts had lost their sensibility. But, notwithstanding no warning is given of the mischief in progress, it runs its course, terminating in death as certainly and rapidly as if indicated by the common train of symptoms. The analogy may hold good as regards heat or cold in their influences on the constitution of the insane: whether felt or not, excess or deficiency of temperature may be as injurious to them, as to individuals possessed of the most delicate sense of them.

Incidentally, I would remark, that in this class of maladies in which the ordinary symptoms of associated disease are so often latent, attention to the temperature, as well as to the pulse, may be of great use to the medical attendant. The pulse and the temperature may make him acquainted with the existence of disease, before not suspected, and may lead to inquiry of a minute kind, by which the exact species of the adventitious disease may be discovered. The case of C. T. Lewis, one of the patients in the lunatic asylum, whose temperature was ascertained and given in the preceding table, is a striking example of the use of such observations. On the 4th of August, the temperature under the tongue was found to be 104·5, and his pulse rapid. This called attention to his state; and, although he made no complaint,—his appetite good,—the functions, as far as was known, tolerably well performed, it was inferred he had obscure disease of lungs. He died in less than a month, namely, on the 26th of August. The fatal disease was pulmonary consumption, organically considered, in an aggravated form, but in relation to symptoms, most mild. There were ulcers of larynx without affection of voice;—most extensive disease of lungs, vomicæ and tubercles without cough; ulceration of intestines without diarrhœa; and disease of testes, vesiculæ seminales, and prostate, of a severe kind, equally latent, excepting in the testicle, the hardness and enlargement of which, but without pain, were casually noticed.

XXIX.

OBSERVATIONS ON THE TEMPERATURE OF THE SHEEP IN WINTER AND IN SUMMER.

The majority of the foregoing observations, seem to show that the temperature of man, and birds, and quadrupeds, is somewhat higher in warm weather than in cool weather,—as in the hot atmosphere of the plains within the tropics, compared with the cool air of the mountains. The observations, however, made by our enterprising arctic travellers, during the depth of winter of intense cold, seem to prove that the animals inhabiting the extreme northern regions have a high temperature, perhaps higher than the same animals in a milder atmosphere. In illustration, I shall insert here the results of some trials made on quadrupeds, by inserting a thermometer several inches into the rectum before the death of the animals, commonly taken in traps, for which I am indebted to the Rev. G. Fisher.

1821.					Fahr.	Air at the time.
Nov. 15,	White Fox	-		-	106·75	+14°
Dec. 3,	"	-		-	101·5	— 5°
" —,	"	-		-	100°	— 3°
" 11,	"	-		-	101·25	— 25°
" 15,	"	-		-	99·75	— 15°
" 17,	"	-		-	98°	— 10°
" 19,	"	-		-	99·75	— 5°
1822.						
Jan. 3,	"	(female)		-	104·75	— 26°
" 9,	" A White Hare			-	100°	— 25°
" —,	" White Fox (male)				100°	— 26°
" 10,	"	"	-	-	100°	— 15°
" 17,	"	"	-	-	106°	— 32°
" 24,	"	"	-	-	103°	— 21°
" —,	"	"	-	-	103°	— 21°
" —,	"	"	-	-	102°	— 21°
" 27,	"	"	-	-	101°	— 27°
Feb. 2,	A Wolf	-		-	105°	— 27°

The great difference of temperature of individuals of the same species, may have been owing to a difference in the length of time they had been entrapped.

Considering the highest temperature as nearest the natural temperature, the results are favourable to the notion above alluded to, that very severe cold may promote the production of animal heat. And, theoretically considered, this appears nowise improbable, much less impossible. Several circumstances may be supposed to conduce to it,—such as the very warm, or bad-conducting nature

of the thick winter clothing of the animals, in conjunction with extreme dryness of air,[1]—the great activity of the function of digestion,[2]—the greater activity of the function of respiration, and the consequent greater degree of aëration of the blood. As our fires burn with more intensity in winter than in summer, and give off more heat, so in animals a greater degree of heat may be produced under the circumstances alluded to, when most required.

With the hope of collecting some satisfactory information on this point, I have selected the sheep, as a fit subject for trial, and I shall now record the results which I obtained in winter, and spring, and summer.

On the night of the 22d January, 1838, the wind changed to the north, and the temperature fell below the freezing point. It continued so, constantly, to the 26th of the same month ; at night generally below 22°, and by day, at or below 29°. On the 26th, at noon, at Chatham, I made observations on three sheep, ewes, which, the preceding day, had been driven in from an adjoining marsh, and had been kept, during the night, in a cool out-house, where the temperature was below the freezing point. At the time of the trials the temperature of the open air was about 29°.

1.—Rectum	-	-	-	-	-	104°
Blood flowing from jugular vein			-	-	103°
" from carotid artery			-	-	105°
Base of brain	-	-	-	-	-	104°
Near surface of cerebrum		-	-	-	103°
Left ventricle	-	-	-	-	-	108°
Right ventricle (blood in it)	-	-	-	107°
Liver	-	-	-	-	-	-	107°
2.—Rectum	-	-	-	-	-	104°
Mixed venous and arterial blood from great vessels of neck						105°
Brain at base	-	-	-	-	-	106°
Surface of cerebrum	-	-	-	-	103°
Left ventricle	-	-	-	-	-	108°
Right ventricle	-	-	-	-	-	106°
Liver	-	-	-	-	-	106°

[1] This is very strongly indicated by a circumstance which I heard related by Mr. King, the enterprising and very intelligent medical officer, who accompanied Captain Sir George Back,—how, in the depth of winter, the blankets exposed to the air after being frozen, dried so completely, that, when brought near a fire, not the slightest appearance of vapour was observable.

[2] The journals of our arctic travellers offer very striking examples both of the accommodating power of the human stomach and the strength of digestion. The quantity of animal food consumed, when there is an ample supply, in the high northern regions, is marvellous. I have been informed by Dr. Richardson, that the daily allowance of meat to the servants of the Hudson's Bay Company, is eight pounds a-man.—Their food is entirely animal food.

3.—Rectum	- - - - -	104°
Mixed venous and arterial blood from great vessels of neck	- - - -	105°
Base of brain	- - - - -	107°
Towards surface of cerebrum	- - -	106°
Left ventricle	- - - - -	109°
Right ventricle (blood in it)	- - -	106°
Liver	- - - - -	106°

From ten to fifteen minutes elapsed between the examination of the brain and heart. The temperature of the rectum was tried during life. The head was cut off immediately after the flow of blood had ceased. The heart was not taken out till the skin and abdominal viscera had been removed.

In the first experiment, in which the difference of arterial and venous blood was tried, the trial was carefully made, and the result unobjectionable. The small bulb of the thermometer was introduced into the jugular vein, and the blood flowed freely over it; and, it was placed in the full stream of arterial blood, close to the wound in the carotid artery, after a ligature had been applied to the jugular.

The following observations were made on the 29th of January, between one and two o'clock, P. M., when the air was about 46°. It rained the preceding night; froze early in the morning; after eight, A. M. there was a continued thaw.

1.—Rectum	- - - - -	105°
Venous blood, from jugular vein	- -	104°
Arterial from carotid artery	- - -	106°
Right ventricle of heart	- - - -	105°
Muscle of left ventricle, where thickest	-	108·5
Left ventricle	- - - - -	108°
Brain at base	- - - - -	105°
Near surface of cerebrum	- - -	103°
Interior of globe of eye	- - - -	100°
2.—Rectum	- - - - -	105°
Vagina (mucous fluid flowing from it, period of æstus)	10⁻°
Blood from jugular vein	- - -	106°
" carotid artery	- - -	107°
Base of brain	- - - - -	106°
Near surface of cerebrum	- - -	105°
Right ventricle	- - - - -	107°
Left ventricle	- - - - -	108°
Interior of globe of eye	- - - -	104°

It was my wish to have examined the heart whilst yet pulsating, to endeavour to ascertain if any heat is produced by the muscular contraction of the organ. The chest was opened within five minutes of the division of the great vessels of the neck; but in both instances too late, the heart had ceased to contract.

The second sheep was in an irritable state; the lungs abounded in granular tubercles, yet the animal was in excellent condition.[1]

The next observations were made on the 16th of March, when the temperature of the air was 51°.

1.—Rectum	-	-	..	-	-	106°
Jugular vein	-	-	-	-	-	106°
Blood from carotid	-	-	-	-	106°
Right pleura[2]	-	-	-	-	-	107°
2.—Rectum	-	-	-	-	-	105°
Mixed venous and arterial blood from cervical vessels						106°
Both lungs	..	-	-	-	-	107°
3.—Rectum	-	-	-	-	-	105°
Mixed blood	-	-	-	-	-	107°
Lung	-	-	-	-	-	108°
4.—Rectum	-	-	-	-	-	106°
Mixed blood	-	-	-	-	-	107°
Lung	-	-	-	-	-	109°
Eye	-	-	-	-	-	104°

These sheep, as well as the two preceding, were ewes, about three years old. The sex of the following, the subjects of observation on the 23d of May, is not mentioned in my notes; the temperature of the air then was 63°.

1.—Rectum	-	-	-	-	-	105°
Mixed venous and arterial blood from neck					-	107°
2.—Rectum	-	-	-	-	-	105°
Mixed blood	-	-	-	-	-	107°
3.—Rectum	-	-	-	-	-	105°
Mixed blood	-	-	-	-	-	107°

The next observations were made on the 21st August, between noon and two o'clock in the afternoon, when the temperature of the air was between 70° and 72°.

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[1] The prevalency of tubercles in the lungs of sheep is remarkable. In their granular state, they do not appear to affect materially the health or condition of the animal; even a few small vomicæ seem to have very little effect. I believe it is precisely the same with the human species; many facts which I have collected are in accordance with it. The analogy is instructive and well deserving of being kept in mind.

[2] The temperature of the lung and pleura was found by introducing a thermometer through a small incision in an intercostal space, immediately after the division of the great vessels, whilst the heart was still acting.

On the 18th March, in the instance of a full-grown spaniel bitch, the temperature, in recto, was found to be 105°, and in the right pleura, the lung not wounded, 107°. 30 drops of hydrocyanic acid were now introduced into the pleura; in half a minute the bitch expired, convulsed. The chest was immediately opened, the heart was still acting, the temperature of the right ventricle was 104°, of the left ventricle 107°; both ventricles were distended with blood, which afterwards coagulated.

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1.—Rectum	-	-	-	-	-	105°
Mixed blood	-	-	-	-	-	107°
2.—Rectum	-	-	-	-	-	107°
Mixed blood	-	-	-	-	-	108°
3.—Rectum	-	-	-	-	-	105°
Blood -	-	-	-	-	-	106°
4.—Rectum	-	-	-	-	-	105·5
Blood -	-	-	-	-	-	107°
5.—Rectum	-	-	-	-	-	104°
Blood -	-	-	-	-	-	105°
6.—Rectum	-	-	-	-	-	105°
Blood -	-	-	-	-	-	106·5
7.—Rectum	-	-	-	-	-	106°
Blood -	-	-	-	-	-	106°
8.—Rectum	-	-	-	-	-	106°
Blood -	-	-	-	-	-	106·5

The two last were lambs about fourteen weeks old. The rest were ewes, about three years old, with the exception of No. 3, which was a ram of about the same age. The testes of this animal were comparatively small, little more than half their autumnal size, yet they contained a good deal of spermatic fluid, abounding in the spermatozoa peculiar to the sheep.

These sheep had not been previously driven. The subjects of the following observations, made at one, P. M., on the 28th August, had been driven ten miles early the same morning, and had been exposed to the sun in the market, and in the butcher's yard. They were all ewes, and about three years old; the thermometer at the time was 79°.

1.—Rectum	-	-	-	-	-	106°
Blood in jugular vein	-	-	-	-	106°
Stream of blood from carotid artery			-	-	107°
2.—Rectum	-	-	-	-	-	105°
Blood in jugular vein	-	-	-	-	106°
Ditto from carotid artery		-	-	-	108°
3.—Rectum	-	-	-	-	-	105°
Blood in jugular vein	-	-	-	-	106°
Ditto from carotid artery		-	-	-	107°
4.—Rectum	-	-	-	-	-	106°
Blood in jugular vein	-	-	-	-	105°
Ditto from carotid artery		-	-	-	107°

All these observations were made with the same thermometer, and consequently they are better fitted for comparison. But after comparing them, it does not appear to me that the results are sufficiently consistent to admit of any positive conclusions being drawn from them. They seem to show, that the temperature of the body is a little higher in the warm weather of summer than in the moderate degree of cold of our winter; the heart perhaps being

an exception. The subject demands further inquiry, especially as regards the effects of extreme cold.—I have brought forward such observations as I have made, with the hope that they may lead to others; and I have given them, miscellaneous as they are, believing that they may have some little value in connection with the theory of animal heat generally, which, to be true and permanent, must rest on a very extensive series of facts—or what is identical, be an induction from them.

XXX.

ON THE TEMPERATURE OF SOME FISHES OF THE GENUS THYNNUS.

It is commonly believed and asserted by naturalists that fishes generally, without exception, are cold-blooded; thus Linnæus, in his "Regnum Animale," characterises them in relation to their blood by "Sanguine frigido;"[1] and Baron Cuvier, our latest and highest authority, not only admits it, but also undertakes to show, that it must be so: thus, in the chapter of his "Histoire Naturelle des Poissons," on the general character and essential nature of fishes, he says, "ne respirant que par l'intermède de l'eau, c'est à dire, ne profitant pour rendre à leur sang les qualités arterielles que de la petite quantité d'oxigene contenu dans l'air mêle à l'eau, leur sang à du rester froid."[2]

It was many years ago, viz. in 1816, when on a voyage to Ceylon, that I first met with an exception to this universally received opinion. It occurred in the instance of the bonito (Thynnus pelamys, Cuv. and Valen.), the temperature of which, in the deep-seated muscles, in the thickest part of the fish, a little below the gills, I found to be 99°, when the surface of the sea, from which it had just before been taken, was 80·5, the difference being, the very remarkable one of eighteen degrees and half.[3]

This fact necessarily made a strong impression on my mind, on account of its singularity; at Malta, in 1833-4, when examining the heart and gills of the tunny, of the Mediterranean, (thynnus vulgaris, Cuv. and Valen.) my attention was recalled to it, on finding that the latter were supplied with nerves of unusual magnitude; that the heart, like that of the bonito, was very powerful; that the fish equally, or even more abounded in blood; and that its muscles generally, like those of the bonito, from the same cause, were of a dark-red colour. It immediately occurred to me that its temperature also might be high, analogous to that of the bonito; and the result of careful inquiry amongst the fishermen of most

[1] Systema Nat. Lib. i. p. 12.
[2] Hist. Nat. des Poissons, Lib. i. p. 275.
[3] Vide above.

experience in the tunny fishery, confirmed the conjecture. All who were asked, declared that the tunny is warm-blooded; and one of the most intelligent, when questioned as to the degree of heat, said, it was much the same, or little less than that of the blood of a pig, when flowing from the divided vessels of the neck on being killed; and this man was very competent to give an opinion on the subject, having been much employed in the fisheries on the Sicilian coast, in which the viscera of the fish are considered the perquisite of the common fishermen, and are instantly taken out by them when the fish are caught.

From the tunny, I extended the inquiry to other fish of the same family; and learnt that the analogy holds good applied to all the species of the genus Thynnus, of Cuvier and Valenciennes, with which the Maltese fishermen are acquainted, viz. *T. brevipinnis*, *T. thunnina*, *T. alalonga*, and the two already mentioned; all of which abound in blood, have a powerful heart, red or reddish muscles, and also, as I have ascertained by particular examination, have their gills amply provided with large nerves. Not having been able to procure any of these fish alive, their exact temperature I have not been able to determine; but, from the reports of the fishermen, it would appear that the common tunny is the warmest of the species, and in accordance with this, I have found its branchial nerves proportionally largest.

These nerves, immediately after quitting the cranium, enter or swell out into ganglia of considerable size, more or less connected together, from whence five principal trunks proceed; the first four chiefly to the branchia, and are the respiratory nerves; the fifth, the lowest, to the œsophagus and stomach, and the parts adjoining. In point of magnitude, these respiratory nerves almost rival the electrical nerves of the torpedo, and their origin is very similar, and their direction and associations, but with this marked difference between them, that the nerves of the torpedo are entirely destitute of ganglia.

The respiratory nerves of the other species of thynnus which I have examined, very much resemble the preceding. Those which are smallest belong to the thynnus brevipinnis; and yet even in this fish, in comparison with fishes of other tribes, the respiratory nerves are large, and their ganglia considerable. This fish, perhaps, may be considered a link between the tunny family and the mackerel, on one side, and the pelamides, on the other; and the respiratory nerves of one of each of these genera which I have dissected, viz. of the scomber pneumatophorus, and the pelamys sarda, have approached in magnitude those of the thynnus last mentioned. What the temperature of these fish is, I have not had an opportunity of determining by trial; according to the statement of the fishermen I have consulted, they are cold-blooded. Reasoning from analogy, it may be conjectured that they will be found to be of somewhat higher temperature than other fishes less amply supplied with respiratory nerves.

As regards the rationale of the high temperature of the species of thynnus, there appears to me less difficulty than in accounting for the electrical power exercised by the torpedo and gymnotus. The peculiar function of the latter is performed by means of a particular organ, the most striking feature of which is a vast apparatus of nerves; but this organization bears little or no analogy to any other natural, or to any artificial process hitherto known by which electricity is generated. Not so the respiratory apparatus and associated organs in these fish of high temperature; they are essentially analogous to those of the warm-blooded animals of the other two classes, and hardly more different from the respiratory apparatus and associated organs of the mammalia, than those of the mammalia are from those of birds. The function of respiration in water is commonly considered similar to that in the atmosphere; the same change, it is supposed, takes place in the blood in both instances; the same change is ascertained to take place in the air of the water and of the atmosphere; and increase of temperature in one case and the other is referred to the conversion of carbon into carbonic acid. The difficulty is not as regards the kind of effect, but the degree of that effect; not an augmentation of one or two degrees above the temperature of the surrounding medium, but of many degrees. A consideration of some of the peculiarities of these fishes may help to diminish this difficulty, which I have little doubt will be removed entirely, when we are better acquainted with their structure, and better acquainted with all the sources of animal heat.

The most important peculiarities, are, I believe, chiefly the following:—a large and powerful heart; abundance of blood; red particles in abundance; large gills, and a very large apparatus of branchial nerves. These, all, may be considered as concerned either directly or indirectly in the generation of heat. And, the circumstances for the preservation of heat are scarcely less remarkable,—as the manner in which the gills are defended by peculiarly strong opercula, abounding in fatty matter,—and the deep situation of the principal blood-vessels, surrounded by thick muscles,—and in addition, the aorta, or great systemic arterial trunk, surrounded by the principal abdominal viscera, the kidneys, stomach, and liver.

Moreover, the habits of these fish, may in some measure contribute to their high temperature. They are frequently to be seen near the surface, and seem to have a delight in springing into the atmosphere. Aristotle, speaking of the tunny, says, "of all fish, it most enjoys warmth, and on that account swims near the surface and frequents sandy shores:"—I quote from the old Latin translation of Theodore Goza. "Thunni omnium maxime piscium gaudent tepore et ob eam rem arenam et littora adeunt; per summa etiam maris innatant quo teporis potiantur:"[1]—confounding (as I

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[1] De Hist. Animal, lib. viii. cap. xix.

believe in this instance he has done) fondness for warmth, with the habits connected with its production.

In this enumeration of circumstances, which may contribute to the high temperature of these fish, both as regards its generation and preservation, I have intentionally been very general; in the present state of our knowledge I apprehend it would be useless to be more minute. It is not impossible that these fish may possess means for generating heat peculiar to themselves, and of which at present we have no adequate idea. And the situation of the kidneys, of which a considerable portion is even higher than the stomach and posterior to the gills, of large size, and abounding in blood, and well supplied with nerves, would lead to the conjecture, that these organs, in the function of imparting heat, may possibly act a part. Still, however, reflecting on the great proportional size of the branchial nerves, and guided by analogy, it is difficult to resist the conclusion, that they are not principally concerned in the performance of the function in question, and that these nerves, as means, are so very ample on account of the element inhabited, and the proportionally greater energy of function required to produce the same effect of elevation of temperature in the water and in the atmosphere. On any other view, it seems difficult to account for the branchial nerves of these fish being proportionally very much larger than the pulmonary nerves of the mammalia, and vastly larger than those of birds, of all animals the warmest.[1] Whether there is any immediate relation between the ganglia and the branchial nerves, and the generation of heat in these fishes is uncertain, and must necessarily remain so, as long as there is any doubt concerning the use of ganglia. The absence of ganglia on the principal nerves of the lungs of man,[2] and I believe of the

[1] The size of the pulmonary nerves of birds, and indeed of their respiratory nerves generally, as far as my observations have extended, is so small as to be truly astonishing, compared with their very high temperature. And, on the hypothesis of nervous influence being essential to the production of animal heat through the agency of respiration, the necessary inference seems to be, that birds require less of this influence, than any other description of warm-blooded animals; which may be owing, perhaps, to their peculiarities of structure, both in relation to the diffused aërial means they possess of generating heat, and their peculiar means, in their covering of feathers, of preserving it; and, owing probably further, to their less expenditure of it, from the peculiarities of some of their principal secretions, especially those of the kidneys, skin, and lungs; their kidneys secreting 'an almost solid urine, their skin exhaling little moisture, and that not in sweat, but entirely by insensible perspiration, and their lungs though exhaling more, from the nature of the function they perform, yet less than might at first be supposed, part of the aqueous vapour contained in the air expired being, I believe, condensed before it enters the atmosphere, by the trachea, mouth and beak, which are always comparatively cool. And in accordance with this view, from the experiments of Messrs. Allen and Pepys, it appears, at least in one instance,—that the pigeon, one of the warmest of birds, consumes in relation to its bulk, even less oxygen, or produces less carbonic acid, than a quadruped, the guinea-pig, the temperature of which is several degrees lower.

[2] Haller, speaking of the great sympathetic nerve, which in man is so

mammalia generally, and of many birds, would lead to the infer-
ence, that the nerves in the instance under consideration, rather
than the ganglia are chiefly instrumental, and that the latter are in
some way subservient to the former,—but whether for giving sen-
sation to the branchia, or for imparting extraordinary secreting
power, so as to change the blood, or for some other purpose, re-
mains to be ascertained.

In conclusion, I would beg to state some desiderata,—points,
connected with the subject of these warm-blooded fishes, which
may be considered as deserving of further inquiry.

1. The exact temperature of different species of the tunny
family.

2. The temperature of the blood, coming from and returning to
the gills.

3. The specific gravity of the blood of these fishes; and the
proportion of serum, fibrin, and of red particles which it contains.

4. The minute peculiarities of their structure.

5. The peculiarities of their habits.

It seems not improbable, that the investigation, if followed up,
may not only throw light on the function of respiration in these
fishes, and on the production of their high degree of temperature,
but also that it may aid in elucidating some obscure parts of the
theory of respiration in general, in connection with that of animal
heat,—and especially the question, whether any oxygen is absorbed
by the blood in the lungs and carried into the circulation.

XXXI.

OBSERVATIONS ON THE TEMPERATURE OF THE HUMAN BODY AFTER DEATH.

It has not come to my knowledge that any precise thermome-
trical observations have hitherto been made on the human body
after death. I was induced to pay attention to the subject, from
having, in one instance, whilst engaged in conducting a *post*

amply provided with ganglia, says, " In pectore notabiles ramos paucos edit,
neque memini me alicujus momenti truncos vidisse qui ad nervum octavi
paris accederunt, etsi ejusmodi nervi illustribus viris visi sunt."   Element.
Phys. iv. p. 260.   And, according to Sir Charles Bell's views of the nervous
system, none of the respiratory nerves are ganglionic nerves.   Sir Edward
Home (Phil. Trans. for 1825, p. 257.) has endeavoured to associate the pro-
duction of animal heat directly with ganglia; but the instances he has
adduced seem liable to great objection; and the fact, that the great sympa-
thetic nerve in birds is comparatively little developed, even less than in some
reptiles, and destitute of large central ganglia, such as the semilunar gan-
glion in the mammalia (at least in every instance I have carefully sought for
them, I have been unsuccessful) seems fatal to his hypothesis.

mortem examination, found the deeply seated parts of a degree of temperature uncommonly elevated. As the inquiry needs as much accuracy as possible, I shall first relate the simple facts, and afterwards offer some reflections on them. It will be necessary to notice the cases in which the observations were made. This I shall do as briefly as is compatible with the objects in view. It may be premised, that they all occurred in Malta, in our military hospitals in Valetta, during a period of six months, namely, from July 1828, to the following January, and that they were all of soldiers belonging to our regiments, serving in that island; and, further, that the bodies, in every instance, almost immediately after death, were removed to a large, airy, and comparatively cool room, the old laboratory of the hospital of the knights, where they were to be examined, and where they were generally covered merely with a sheet, and placed on a table of wood.

1. Aged 23 years; was admitted into hospital on the 30th of July, 1828, labouring under symptoms (as it was supposed) of acute rheumatism, having severe pain, first in one shoulder, then in the other, followed by pain in the hips, attended with much pyrexia, and a very rapid pulse. He died on the 6th of August, a few minutes after seven, a. m. As the weather at the time was hot, and dead animal matter putrefied rapidly, it was necessary to inspect the body as soon as possible. Accordingly, it was examined three hours and a half after death; when the temperature of the air of the room was 86°. The appearances most remarkable, discovered on dissection, were extensive collections of matter in the right shoulder, amongst the muscles on each side of the spine of the scapula, with sinuses extending to the axilla, and round the capsule of the shoulder joint; and, a lesion of the same kind, and as extensive, in the left hip, close to the head of the femur, spreading through the glutei muscles; and marks of incipient inflammation (as ecchymosis) in the right hip. The viscera were apparently sound. The right cavities of the heart, and the great vessels, were distended with liquid blood. The body was slender, but not emaciated. Its surface was warm; and deeply seated parts felt very hot, imparting a disagreeable sensation, almost like that of burning, to the hand, in contact with them. The thorax was first opened, and afterwards the abdomen. After partial exposure of the surface of the contents of these cavities to the air for about ten minutes, a thermometer was procured. Placed under the left ventricle of the heart, it rose to 113°; and, under the liver, in contact with the lobulus Spigelii, to 112°.

2. Aged 27 years; a stout robust man, died suddenly in barracks, on the 6th of August, at about half-past eleven, a. m. The body was examined at five, p. m. There was a good deal of reddish fluid in the ventricles, and at the base of the brain. The lungs were distended with black liquid blood, some of which had passed into the bronchia. There was very little blood in the cavities of the heart. The temperature of the air of the room was 86°. As

soon as the cavities of the thorax and abdomen were opened, the bulb of a thermometer was in succession placed under the left ventricle of the heart, and under the lobulus Spigelii of the liver; in the former situation it rose to 108°; in the latter to 107°.   About a quarter of an hour afterwards, introduced into the substance of the right lung, gorged with extravasated blood, it was 105°.

3. Aged 25 years; a large, stout, muscular man, was admitted into hospital on the 19th of August, with symptoms of continued fever.   On the night of the 22d, he became delirious; but only for a short time.   On the evening of the 24th he died suddenly and unexpectedly.   The body was examined four hours and a half after death, during which time the temperature of the air of the room had been between 78° and 80°.   No organic disease was discovered on dissection, excepting of a chronic kind, and slight, and not explanatory of the fatal event; indeed, the abdominal and thoracic viscera, and the brain, were unusually sound.   Notwithstanding the body was large and fat, its surface was cold : but the deeply seated parts were warm.   A thermometer introduced under the left side of the heart, just after the pericardium was opened, rose to 97°, and under the liver close to the vena portæ, to 101°. The brain had been previously examined; the cavities of the thorax and abdomen were opened about four hours after death.   It may be remarked there was a great deal of blood in the cavities of the right side of the heart, and a small quantity in the left.

4. Aged 21 years; rather a large muscular man; was admitted into hospital on the 21st of August; and died on the 11th of September, of acute dysentery.   The examination of the body was commenced about four hours and a half after death, and the thermometrical observations were made about half an hour later; the temperature of the air at the time was about 82°.   The viscera in general were sound, excepting the large intestines, which exhibited the worst effects of dysentery.   The trunk was warm; the extremities cold and rigid.   A thermometer placed under the lobulus Spigelii of the liver, rose to 103·5, and under the left ventricle of the heart to 103°.

5. Aged 23 years; a stout, muscular, well-made man; was admitted into hospital on the 9th of October, with symptoms of acute rheumatism, which were followed by those of remittent fever, but not very strongly marked, attended with a low degree of pyrexia, rather rapid respiration, and a pulse little accelerated.   He died rather suddenly on the 16th of October.   The body was examined fourteen hours after death; when the limbs were rigid and cold.   The temperature of the air at the time was 69°.   Some coagulable lymph was found adhering to the back part of the pleuræ; a corresponding portion of each lung was apparently hepatized; and, both the liver and spleen, and also the heart, were of a morbid degree of softness, and easily broken; more especially the two former, which were nearly of pultaceous consistence.   A

thermometer placed under the lobulus Spigelii of the liver, as soon as the cavity of the abdomen was opened, rose to 88°.

6. Aged 24 years; a large stout man; was admitted into hospital on the 12th of November, 1827, with symptoms of acute rheumatism; and died on the 4th of November, 1828, eighteen days after amputation of the thigh, on account of vast and complicated disease of the lower part of the limb involving the bone and knee joint, and twelve days after the ligature of the femoral artery, on account of secondary hæmorrhage from the stump. The body was not emaciated. It was examined twelve hours after death; when the temperature of the air was 68°. The branches of the femoral vein were found plugged up with coagulable lymph; the trunk of the vein was lined with coagulable lymph, and there was pus, or, a puriloid fluid, in the iliac vein. A thermometer placed under the lobulus Spigelii of the liver, rose to 91°, and under the heart to 93°.

7. Aged 23 years; a stout, well-made man; was admitted into hospital on the 9th of November; and died on the 29th of same month, of acute dysentery. The body was examined three hours after death, when the temperature of the air was 66°. A large abscess was discovered in the liver, of which there were no symptoms during life; and the colon throughout exhibited, in a high degree, the ulceration and disorganization peculiar to dysentery. A thermometer placed under the lobulus Spigelii of the liver, rose to 96°; in the substance of the right lobe (in which the abscess existed) to 98°, and the same under the left ventricle of the heart. About five minutes after, when placed in the posterior horn of one of the lateral ventricles of the brain, it fell to 90°.

8. Aged 33 years; a tall, muscular, well-made man; was admitted into hospital on the 30th November; and died on the 1st of December, of apoplexy, with hemiplegia. The symptoms were most distinct and violent; a large quantity of blood was abstracted, and energetic treatment employed. The body was examined two hours after death; during which time, the temperature of the room was between 60° and 64°. The cause of the symptoms was not discovered by careful dissection. No blood was found extravasated on the brain; not more fluid than is usual in the ventricles; no softening of any part of the organ; no tumour making pressure on it; and, not a greater degree of fulness of its vessels and redness of its membranes, than is frequently met with in cases in which the powers of sense and intellect have remained unimpaired to the last. A thermometer, placed under the lobulus Spigelii of the liver, rose to 86°, and under the right ventricle to 88°, immediately after the cavities were opened.

9. Aged 23 years; a tall thin man; was admitted into hospital on the 13th of September, and died on the 3d of December, of phthisis pulmonalis, of slow progress, and mild symptoms. During the last month, there was no cough, no dyspnœa, no uneasy feeling, a slight diarrhœa, gradual wasting, and loss of strength. The body was examined five hours after death; during which time, the temperature of the air varied from 59° to 64°. The lungs abounded

in small gray firm tubercles, and in small curd-like tubercles, and contained a few vomicæ; the principal part of their substance was crepitous. The lymphatic glands of the thorax and abdomen, generally, were enlarged and softened in different degrees. There was an ulcer in the inner coat of the duodenum; many ulcers on this coat of the jejunum; and the ileum and large intestines abounded in ulcers, many of them deep and large. A thermometer introduced into the substance of the liver rose to 90°: it indicated the same degree of heat placed under the lobulus Spigelii, and under the right ventricle of the heart, immediately after the thoracic and abdominal cavities had been opened. The skull-cap as soon as possible was removed, and immediately the membranes of the brain were divided by a deep incision, penetrating into the centre of the cerebrum, into which the thermometer being placed, it instantly fell to 82°.

10. Aged 28 years; rather small, but a well-made man; was admitted into hospital on the 23d of December, with symptoms of an obscure kind, seeming to indicate disease of the liver. He had occasional pain in the region of that organ, and also of shoulder; a rigor now and then; a rapid pulse; but little pyrexia, or loss of appetite, or wasting. He died on the 31st of January. The body was examined seventeen hours and a half after death, during which time the temperature of the room was from 58° to 60°. The liver, large in its dimensions, was found to be excavated in a singular manner, not unlike a tuberculated lung, in its advanced stage of suppuration, abounding in vomicæ. The excavations in the liver were extremely numerous and labyrinth-like, so irregular were they in form, and communicating with each other so irregularly. Their walls were in many instances columnar, and the cavities intersected by columns containing blood-vessels, and by blood-vessels almost bare. Their contents were a viscid bilious purulent fluid; and its yellow colour was apparently derived from bile discharged from the ulcerated ends of the branches of the biliary duct, indicated by bright dark-yellow spots on the surface of the excavations. In the situation of the vena portæ, there was a large sinus, which was ruptured by introducing the thermometer under the lobulus Spigelii; it discharged a fluid similar to that described above. On minute examination, this sinus, lined with ragged coagulable lymph, and about thrice the size of the vena portæ, was found to be that vein itself vastly diseased: and the veins which terminate in the vena portæ were found terminating in it, empty, and their abrupt terminations in this sinus or sac closed with coagulable lymph. The hepatic artery, vein and duct (at least their trunks), were not diseased. The spleen was of usual size and appearance, but unusually compact and hard. A thermometer introduced into the centre of the cerebrum, so soon as the skull-cap was removed, rose to 70°; about half an hour after when placed under the right ventricle of the heart, it rose to 82°, and the same under the right lobe of the liver.

These examples, which I have introduced in the order, in point of time, in which they occurred, are the only ones in which I tried the temperature of the body after death, in a warm climate. They will probably be considered of very unequal interest, comparing those in which an unusually high temperature was noticed, with the majority in which the temperature was no higher than might have been expected.

It would be superfluous on this occasion to enter into any minute disquisition relative to the rationale of the very high temperature observed in the bodies Nos. 1 and 2, in relation to the different degrees of temperature noticed in the other bodies. The question will naturally be asked, Was the extraordinary temperature in one case, of 113°, and in the other of 108°, generated during life or death? I have little hesitation in coming to the conclusion, that it was generated before death, and generated probably in the same way as the ordinary degree of animal heat experienced in health, or the extraordinary degree witnessed in febrile diseases. *A priori*, the effect of the heat-generating process, whatever it may be, can hardly be limited. In many birds, it raises the temperature of the body to 109°, when in perfect health, and in man to 101°, at least in the tropics, without deranging health; and it is easy to conceive, that, by increased activity or energy, it may exalt the temperature to the common febrile height, or to a height greatly exceeding that. But, destitute of life, there does not appear to be in the body any source of heat, any power of generating it, that we are aware of. Putrefaction had not taken place in these bodies; I believe I may say it had hardly obscurely commenced. Even if it had, and had made progress, and were it even at its greatest height of activity, it is doubtful if it would be equal to the production of the effect in question?

It may be matter of regret, that the temperature in the two first examples, was not ascertained before as well as after death. It is only known that the skin of No. 1, the day he died, was pungently hot—the expression I believe used by Dr. White, the zealous medical officer under whose care he was. As the weather at the time was oppressively hot, and the sensations of those in health, as well as of those labouring under disease, distressing more or less in relation to heat, less attention naturally would be paid to the degrees, and there would be the more difficulty in distinguishing them, and saying this is natural,—that morbid.

As the facts stand (and I trust I may venture to call these observations such), they may tend to put medical inquirers on their guard relative to the extreme limits of degree of animal temperature, and especially of the blood and deeply seated parts. If the temperature of a body, as in No. 1, be 113°, three hours and a half after death; and, if generated during life, before death it must have been still higher. Let us suppose some part of this individual inflamed, and observations made on the heat of the inflamed part; it is easy to conceive that its temperature might be found unusually

high,—and greatly exceeding the average temperature of the body in health, or in ordinary disease. If it were immediately concluded that this was an instance of the local generation of heat in the inflamed part,—it would be an assumption, and might be considered an error. And, have not observations of this kind been made and published, and adduced as arguments in reasoning on animal heat, as regards its theory?

Relative to the majority of the examples in which the temperature of the dead body was ascertained, I shall avoid making many reflections at present: I shall be well pleased if they induce others to continue an inquiry which may lead to interesting results, both as concerns physiology, and perhaps pathology.

The temperature of a part, perhaps, may prove a safe criterion, *cæteris paribus,* of the quantity of blood which passes through it in a given time. According to this test, it may be inferred, that the supply of blood to the brain, in the ordinary estimates of it, is over-rated.

May not the temperature of the deeply seated parts, after death, give some insight into the manner in which death has occurred, or help to elucidate its cause? In example No. 9, the temperature of the heart and liver was low. Was this connected with the state and function of the lungs, during the latter period of life, when much organic disease existed in them; when their functional action appeared to be feeble, and the quantity of heat generated small?

Additional observations on the temperature of the body after death.

The subject discloses, besides what have already been adverted to, other openings for curious and perhaps useful inquiry, especially in medical jurisprudence.

It may often be a question, how long a body has been dead. By attention to its temperature, particularly of the deep-seated parts, taking into consideration the circumstances affecting temperature, probably, in most instances, an answer may be given, approximating to the truth, and which may prove of considerable use in evidence.

As the preceding observations were all made in a comparatively warm climate,—it appeared to me desirable to extend them in this country,— particularly in relation to the application above mentioned. I shall now detail such additional ones as I have had an opportunity of collecting, all of which have been made during the year 1838, in the General Hospital, at Fort Pitt, Chatham, on subjects similar to those on which the trials were instituted at Malta— being all soldiers. It may further be premised, that immediately after decease, the bodies were removed from the wards, and were kept in a room, covered merely with a sheet, in which the temperature was seldom more than 4° or 5° above that of the open air.

1.—Aged 26 ; died of pulmonary consumption on the 17th January, at 8 A. M. and was examined twenty-eight hours after.

Emaciation was not great. The lungs were extremely diseased, presenting cavities, numerous tubercles, induration and œdema, complicated with ulceration of trachea and large intestines. The quantity of blood generally was small. The temperature of the parts was tried in the order in which they are specified.

Between the hemispheres of the brain in contact with the corpus callosum, immediately on removing the calvaria - 46°
Under the left ventricle of heart (about an ounce of fluid in pericardium) - - - - - 52°
In the cavity of the right ventricle which contained a good deal of dark blood - - - - - 50½°
In that of left ventricle which was nearly empty - 51°
Under lobulus Spigelii - - - - - 50°
In substance of liver - - - - - 50°
In thigh, close to femoral artery, just before it dips under the sartorius muscle - - - - - 42°
Beneath the integuments of sole of foot - - 39°

The thermometer during the whole time in the open air was at or below 30°; and in the morning early was as low as 13°.

2.—Aged 19; died of pericarditis on the 20th January, at 8 P. M. and was examined sixteen hours after. The pericardium was distended with 20 ounces of turbid serum, from which pus subsided on rest. There was much wasting; little blood; 18 ounces of serum in the right pleura; 32 in the peritoneal cavity; œdema of limbs.

Longitudinal sinus, containing fluid blood - - 30°
Between the hemispheres in contact with the corpus callosum 51°
In lateral ventricle - - - - - 51°
At base, under pons varolii, the brain in situ - - 57°
In spinal canal, in which, and in ventricle, pretty much fluid - - - - - - 57°
In serum of right pleura - - - - 63°
In fluid of pericardium - - - - 65°
Under the left ventricle - - - - 67°
In fluid of abdomen - - - - - 63°
Under œdematous integuments of thigh, near the artery as before - - - - - - 47°
Near the artery in calf of leg - - - - 34°
Under integuments of dorsum of foot - - - 32°
              of sole of foot - - - 31°

The atmospheric temperature during the whole time was low, at night below 20°, by day, never above 30°; the temperature of the room where the body was placed, was 30°; when examined, it was about 40°.

3.—Aged 29; died of pulmonary consumption on the 26th January, and was examined eighteen hours after. There was great emaciation; little blood; there were the ordinary lesions in the lungs, with empyema of left pleura, and ulceration of ileum and cœcum.

In longitudinal sinus (calvaria unusually thin) -	55°
In lateral ventricle - - - -	55°
In upper part of spinal canal - - -	55°
Under integuments of sole of foot - -	43°
of dorsum of foot - -	44°
of thigh close to artery -	52°
In purulent fluid of left pleura (46 oz.) -	64°
In right ventricle, moderately distended with blood, partly liquid, which afterwards coagulated -	63°
In left ventricle, empty - -	64°
In central substance of right lobe of liver -	65°

The observations were made in succession, without interruption, in the space of five minutes. The temperature of the room the greater part of the time was about 40°.

4.—Aged 18 ; died of pulmonary consumption, on the 9th February ; and was examined twenty-nine hours after. There was great emaciation ; the lungs were very extensively diseased,—tuberculated, containing cavities, partially œdematous, partially hepatized ; and the larynx, ileum and cœcum were ulcerated.

In third ventricle of brain - - -	50°
Under medulla oblongata - - -	50°
In spinal canal, at its upper part - -	49°
Under lobulus Spigelii - - -	56°
In fluid of pericardium (about two ounces) -	57°
In right ventricle, which contained much blood -	57°
In left, nearly empty - - -	57°
Under integuments of thigh, close to femoral artery -	49°
Under integuments of sole of foot - -	47°

The temperature of the room, during the time, was between 38° and 48°.

5.—Aged 17 years ; died the 20th of February, of peripneumonia ; and was examined twelve hours and half after. The right lung was greatly condensed, and weighed three pounds and half; there was sub-emaciation and œdema of the lower extremities.

Between the hemispheres, on corpus callosum -	57°
In lateral ventricles - - -	57°
In upper part of spinal canal - -	59°
Under the heart - - -	67°
Right ventricle, moderately distended fluid with blood, which afterwards coagulated -	64°
Left ventricle, empty - -	68°

6.—Aged 24 years ; greatly emaciated ; died on the 30th of March, of peritoneal inflammation, with ascites, variously complicated, and was examined fourteen hours after.

In longitudinal sinus - -	62°
In substance of right hemisphere -	66°
In lateral ventricles - -	64°
In upper part of spinal canal - -	64°

In the fluid in the abdomen (seven pounds)   -   -   73°
In right ventricle of heart   -   -   -   73°
In vena cava ascendens under liver, which weighed five
    pounds   -   -   -   -   -   77°

7.—Aged 35 years; died 17th August, of malignant tumour in face, with great destruction of the surrounding parts, including a considerable portion of each jaw-bone. The body was extremely emaciated, and contained very little blood. It was examined five hours after death; the temperature of the air of the room at the time was 68°.

Between the hemispheres of cerebrum on corpus callosum 86°
In fluid of lateral ventricles, which was in rather large
    quantity   -   -   -   -   -   87°
In cavity of pelvis   -   -   -   -   90°
Under lobulus Spigelii of liver   -   -   -   90°
Under heart, within the pericardium -   -   -   94°
Close to femoral artery, midway between hip and knee   77°
Under integuments of sole of foot   -   -   67°

8.—Aged 40 years; died 18th August, of chronic dysentery, just after his return from the West Indies. The body was sub-emaciated; it contained pretty much blood, and especially the abdominal viscera. The examination was made four hours and half after death. The temperature of the air at the time was 68°.

Between the hemispheres on corpus callosum   ·   85°
In lateral ventricle, which contained but little fluid   -   86°
In upper part of spinal canal -   -   -   -   87°
Under lobulus Spigelii of liver   -   -   -   97°
In cavity of pelvis   -   -   -   -   97°
Under left ventricle   -   -   -   -   93°
In right ventricle, in which a good deal of frothy blood   92°
In left ventricle, empty   -   -   -   -   95°
In middle of thigh, close to great vessels   -   -   82°
Under integuments of sole of foot   -   -   -   69°

9.—Aged 22 years; died 19th August, (nineteen days after the amputation of his fore-arm, on account of disease of the bone), of complicated and severe organic disease,—chiefly latent,—as tubercles and vomicæ in the lungs,—abscess under the scapula, communicating with the shoulder joint, &c. The body was greatly emaciated, and contained very little blood. The examination was made eighteen hours after death; the temperature of the air at the time 68°.

Between the hemispheres of the brain on the corpus callo-
    sum   -   -   -   -   -   -   68°
In lateral ventricle   -   -   -   -   68°
At base of brain, where there was a good deal of fluid, and
    in the upper part of spinal canal   -   -   -   68°
Under liver   -   -   -   -   -   71°
Under heart, within the pericardium   -   -   -   72°
In left ventricle, which was empty   ·   -   -   73°

In right, in which was some blood   -   -   -   72°
In thigh, mid-way between knee and hip, close to femoral
    artery   -   -   -   -   -   -   65°
Under integuments of sole of foot   -   -   -   64°

10.—Aged 26 years; died 19th August, of pulmonary consumption. The body was sub-emaciated, and contained pretty much blood, which was not coagulated. The examination was sixteen hours after death; the temperature of the room was 72°.

Thermometer on corpus callosum   -   -   -   67°
Under liver   -   -   -   -   -   -   81°
Under heart   -   -   -   -   -   -   81°
Under left ventricle   -   -   -   -   -   80°
In right ventricle   -   -   -   -   -   80°
In thigh, close to femoral artery   -   -   -   72°
Under integuments of sole of foot   -   -   -   66°

These observations require little comment. Their application to the use of medical jurisprudence, in connection with the question referred to, is obvious. Although limited in number, (and it would be tedious to extend them,) they may enable the inquirer, instituting similar thermometrical trials, and reasoning analogically, to arrive at a tolerably positive conclusion, in doubtful cases of death, as to the time which may have elapsed, between the fatal event and the post mortem examination.

Much judgment, however, and nice discrimination may be requisite on the part of the medical man, in appreciating the circumstances likely to modify temperature, so as to enable him, when called on for his opinion, to give one which will be satisfactory to the legal officers, and to himself, on reflection.

XXIV.

AN ACCOUNT OF TWO CASES OF PNEUMATHORAX WITH EXPERIMENTS AND OBSERVATIONS ON THE ABSORPTION OF AIR, BY SEROUS AND MUCOUS MEMBRANES.

The two following instances of Pneumathorax were communicated to the Royal Society, not as medical cases, but on account of the physiological questions connected with them, and the experiments of the same kind which they gave rise to. This alone could have entitled them to a place in the Philosophical Transactions, in which they were first published, in the volumes for 1823 and 1824; and it is this consideration which induces me to include them in the present work.

CASE I.—Abraham Iredill, a private soldier, of the 7th regiment of foot, aged 30, was admitted into the general military hospital, at
12—d   22 day

Fort Pitt, Chatham, on the 15th of January, 1823, labouring under phthisis pulmonalis, which proved fatal on the 11th of February.

The disease in this its last stage exhibited some peculiarities, the cause of which, referrible to pneumathorax, was not discovered until after death, for the case being considered hopeless, and the weather being very cold, the requisite examination of the chest was intentionally omitted.[1] The body was inspected fourteen hours after death. The right side of the chest exhibited a great degree of fulness; and it emitted, when struck, a hollow sound. On carefully opening the abdomen, the diaphragm was found protruding into the right hypochondrium, exhibiting a convex surface, almost conical, instead of concave; and it was tense and tympanitic. The right lobe of the liver was pressed into the epigastrium, and rested on a portion of the stomach and duodenum, and on a part of the transverse colon. Owing to the pressure of the liver, the pyloric portion of the stomach was removed from its natural situation to the left iliac region, where it rested on the upper part of the sigmoid flexure of the colon; and owing to the same pressure, the small intestines generally were driven downwards and more or less displaced.

The body having been immersed in a bath, a small opening was made with a scalpel under water, into that part of the right pleura best adapted by situation to allow of the escape of air. Air issued out in abundance, and with much force; 212 cubic inches were collected in receivers, and about thirteen cubic inches escaped, making altogether the enormous volume of 225 cubic inches. The air collected was set aside for analysis, and the body having been replaced on the table, a portion of the ribs was removed from the right side to admit of the examination of the chest, the water which had rushed in to supply the place of the air having been carefully taken out and preserved.

The inner surface of the right pleura was covered with a thin layer of coagulable lymph. The right lung was exceedingly compressed; it adhered closely to the upper part of the pericardium, and loosely to the posterior part of the chest (about the sixth and seventh ribs) by a few strong bands.

On inflating the lungs with a double bellows through an opening into the trachea, the right lung became much expanded, and air was found to pass freely from the lung into the pleura through an ulcerated opening in the upper part of the superior lobe.

The right lung was carefully dissected out. In the upper part of its superior lobe, a tubercular excavation or vomica was found, of the capacity of about four ounce-measures, communicating with the aspera arteria by a large bronchial tube, the ulcerated end of which terminated in the side of the excavation opposite to the openings by which the vomica communicated with the pleura.

[1] At this time, the stethoscope was not employed in the general hospital; I first saw this excellent instrument the following autumn in Edinburgh, and then commenced its use.

On examining minutely the communication between the cavity of the chest and the lung, a kind of valvular structure was discovered, allowing of air being pumped into the pleura in the act of inspiration ; but not of its escape in expiration, to which no doubt was owing the great accumulation of air.   Even at the risk of being tedious, I shall attempt to convey some idea of this structure. Between the false membrane of the vomica and the pleura, there was a small irregular sinus, not exceeding an inch in diameter, the sides of which, though not adhering, of course were in contact, or very nearly so.   This sinus was the channel of communication, and contained the valvular structure alluded to.   It opened into the cavity of the chest by a hole in the pleura pulmonalis, about the size of a crow-quill, and into the vomica by three smaller holes in the substance of the lung, not corresponding with the former, so that a probe could not be passed from one into the other in a straight line ; and, consequently, when the surfaces of the sinus were pressed together by the compression of the air in the pleura, in the act of expiration, the passage through which the air entered was closed and its exit prevented.

A very brief notice of the other morbid appearances will suffice. Besides the vomica described, in the right lung, this viscus contained small tubercles, few in number in the inferior lobe, but abundant in the superior.   The largest of them did not exceed in size a common pea, and the smallest were not larger than a mustard seed. The smallest were translucent ; the larger were of different degrees of opacity ; all of them were solid ; none had suppurated.   The left lung was free from adhesion.   Like the right it contained numerous small tubercles, which had made very little progress. The bronchia were redder than natural, as was also the lower part of the trachea.   Three ounces of serum were contained in the pericardium, and a larger quantity of fluid than usual in the ventricles of the brain.   No air could be detected in the blood vessels, or in the cellular membrane of any part of the body.

To return to the contents of the right pleura ; the water taken from this cavity (that which entered when the air was discharged) was turbid, as if from the admixture of pus.   After resting twenty-four hours in tall glass jars, a white sediment formed, which, carefully separated by decantation, was about an ounce in quantity.   It had the appearance of pus, and exhibited the properties of pus when examined by the most approved tests :—thus, it became viscid with a solution of muriate of ammonia ; it was soluble in sulphuric acid, and precipitable by dilution with water ; and it produced coloured rings when placed between two surfaces of glass, and held before a candle, according to the method recommended by Dr. Young.   The decanted fluid, examined by a solution of corrosive sublimate, and by evaporation, was found to contain serum ; and judging from the extract it afforded, it was about 11 ounces in quantity, half an ounce of the decanted water having yielded, when evaporated, 2·2 grains of dry residue.

The air collected from the pleura, had not the least fetor, nor indeed any smell. It extinguished flame, and was not inflammable. Examined by means of lime-water and phosphorus, (which was sublimed in it without effect,) 100 parts of it were found to consist of 8 carbonic acid gas, and 92 azotic gas.

Whence this air was derived became a question for consideration. Reflecting on the communication, discovered by dissection, between the pleura and the atmosphere through the medium of the lung, it seemed almost demonstrated, that the air was atmospheric air altered.

The next question was, how the alteration had taken place; what had become of the oxygen which had disappeared; whence the carbonic acid gas with which the azote was mixed?

To endeavour to learn how the oxygen had disappeared, the following experiment was instituted. The right pleura of a dog was inflated with atmospheric air, by means of a double bellows, and the incision through which the air was introduced was closed by a suture. At the end of forty-eight hours, the dog was killed. An hour after death, the pleura was punctured under water, and about 8 cubic inches of air were collected. This air, examined by means of lime-water and phosphorus, was found to contain slight traces of carbonic acid gas, and to consist of 93 parts azotic gas, and 7 oxygen gas. The wound in the pleura was closed by coagulable lymph, and the pleura was found free from inflammation.

The result then of this experiment seems to show that the oxygen was absorbed in greater proportion than the azote, and thus tends to account for the accumulation of the latter gas in the preceding case.

It may be said, that the experiment does not warrant the inference, that any azote was absorbed; and, consequently, that the expression "in greater proportion," is incorrect. The absorption of this gas is probable, however, though not demonstrated in the present instance, as Sir Astley Cooper has found, that common air introduced into the cellular membrane, and into the cavity of the thorax and abdomen of dogs, is, after a certain time, entirely removed by absorption.[1]

Relative to the source of the carbonic acid gas, it is easy to conceive that this gas was formed, or emitted in the air cells of the lungs, as in ordinary respiration; and that mixing with the air inspired, it was received into the pleura. If thus derived, and not from the surface of the pleura by secretion, it seems to follow, that it is less readily absorbable by the pleura, than oxygen. To endeavour to decide this point, the following experiment was made.

About 30 cubic inches of air, consisting of 80 parts common air, and 20 carbonic acid gas, were passed from a receiver into a bladder, furnished at one extremity with a stop-cock, and at the other with

[1] Surgical and Physiological Essays, by John Abernethy, p. 55. London, 1793.

a small trochar; both air-tight. A small incision having been made through the integuments of the right side of the chest of a dog, the trochar was passed through the intercostal muscles into the pleura. The stilette was immediately drawn from the canula into the bladder, and the air of the bladder instantly rushed into the pleura, and, on expiration, was in part forced back into the bladder. The exact quantity of air retained was not determined; it must have exceeded at least ten cubic inches. As speedily as possible the canula was withdrawn, and the external wound carefully closed by suture. The health of the dog was very little impaired by the operation. Two days after, when the animal appeared to be quite well, a similar experiment was made on the left side of the chest, and a mixture, consisting of 75 parts common air, and 25 carbonic acid gas, was introduced into the pleura. The operation had very little more effect than the former. At the end of twenty-four hours, the dog was killed, and immediately examined. About three cubic inches of air only were procured from the left pleura, which were found to consist of

> 18·3 carbonic acid gas,
> 78 3 azotic gas,
> 3·4 oxygen gas;

whilst the air admitted consisted of

> 20·0 carbonic acid gas,
> 63·2 azotic gas,
> 16·8 oxygen gas.

Thus apparently showing, that during a sojourn of three days in the pleura, the oxygen had been absorbed in a greater degree than the carbonic acid gas; and the latter, in a greater degree than the azote. The result of the experiment on the left pleura was very similar; it afforded ten cubic inches of gas, consisting of 25 carbonic acid, 70·6 azote, and 4·4 oxygen. The appearances, on examining the wounds, were satisfactory: the cavity of the chest was free from inflammation, the lungs uninjured, and the cicatrix in the pleura only just perceptible.

The results of these experiments seem to warrant the conclusion, that, in the preceding case, the carbonic acid gas found, was not derived from the surface of the pleura by secretion, or exhalation, but from the respired air, through the ulcerated opening. And, with this remark, I shall dismiss the case of pneumathorax,—the consideration of which, as a medical subject, would not be appropriate in this place.

The power exhibited by the pleura, in the preceding instances, of absorbing gases, and the manner in which it exercised that power, in a greater degree, on one air than on another, and that in no ratio to their solubility in water, appeared to me so interesting and novel, that I was induced to prosecute the subject a little further. With the same apparatus, I made similar experiments on the admission of three other gases into the pleura of dogs, viz.—

hydrogen, nitrous oxide, and nitrous gas,—the results of which I shall briefly describe.

About twenty cubic inches of a mixture, consisting of 57·5 parts carbonic acid gas, and 42·5 hydrogen, were admitted into the left pleura of a dog, in the manner, and with the precaution, already noticed. The health of the animal was not apparently affected. At the end of two days, about thirty cubic inches of a mixture, consisting of 44·5 azote, and 55·5 nitrous gas, were passed into the right pleura. Immediately the dog's breathing became quick and short, but not laborious. It refused to eat, and expired in the evening, at the end of five hours from the time that the air was introduced. The next morning the body was examined. About six cubic inches of air were collected from the left pleura, consisting apparently of 12 carbonic acid, and 88 azote. After the removal of the carbonic acid gas, by lime-water, the residual gas extinguished flame, and was not itself the least inflammable; and hence the inference, that it was azote, or, at least, principally azote, as the presence of a small quantity of hydrogen might be concealed, and escape detection. From the right pleura, about 5 cubic inches of air were procured, which consisted of 6·9 nitrous gas, or air absorbable by a solution of the green sulphate of iron, and of 93·1 azote. On opening the chest, the wounds in the pleura were found closed; the pleuræ were of natural appearance; the substance of the left lung was redder than usual, and that of the right was dark red, and it contained a good deal of blood and serum. The bronchia did not exhibit decided marks of inflammation. The right auricle and ventricle, and venæ cavæ, were distended with grumous blood; and the left auricle and ventricle, and aorta, contained a good deal of liquid blood, which, as well as that of the venous system, had lost its peculiar tint, and had acquired a chocolate hue.

The obvious results of these two experiments, on the same dog, are,—1st, The absorption of the greater part of the carbonic acid gas and the whole of the hydrogen introduced into the pleura, and the appearance, *de novo*, of a considerable quantity of azote; 2d, the death of the animal, in the space of five hours, from the time of admission of the nitrous gas and azote into the opposite pleura; the absorption of the greater part of the former gas, without inflammation of the membrane with which it was immediately in contact, and the production of a peculiar change in the blood.

Results so singular required to be narrowly scrutinised. I have twice repeated the experiment on the admission of hydrogen into the pleura of dogs, and in each instance, after death, I found that the hydrogen had disappeared, and that its place was supplied by a small quantity of azote.

Did the azote found, in these instances, exist in the pleura previous to the experiment?

A remark of Dr. Laennec would seem to countenance this notion. He says;—" M. Ribes assures me, that he has found, on opening the serous cavities of dogs, a small quantity of air con-

stautly to escape."[1]   On the contrary, in opposition to this, are the experiments of Haller and other accurate observers, recorded in the controversy which Hambergerus gave rise to, by reviving and maintaining the opinion of Galen, that air is contained between the lungs and pleura.[2]

In doubt between these contending authorities, with the desire of satisfying myself on the point, I have made some experiments on dogs, the results of which convince me, that, in a healthy state, no air is contained in the pleura of this animal.   When I opened under water the chests of dogs killed by drowning, not the smallest globule of air escaped ; but when the right side of the chest was opened in the atmosphere, an appearance presented, at first favourable to the idea of a little air being contained in the left pleura, for the mediastinum was pressed from the left side towards the right (the body lying on the left side) evidently by air within the transparent membrane.   This appearance, on examination, proved to be fallacious, for the air was found to be not in the left pleura, but in a cavity of the mediastinum communicating with the right pleura, and containing a lobule of the right lung, *dextri pulmonis additamentum*, as Haller calls it, who has noticed this structure in the mediastinum of the dog and many other animals, and pointed it out as one of the principal causes of the erroneous notion which he combated.[3]

Was the azote derived from the blood as an exhalation or secretion ?

Facts might be advanced in favour of this idea.   An exhalation or disengagement of azote appears to have taken place in the experiments of Messrs. Allen and Pepys, when oxygen, nearly pure, was respired.[4]   In the inspection of dead bodies, air has frequently been found in the vessels and closed cavities ; which is probably azote.[5]

[1] A Treatise on the Diseases of the Chest, &c. translated from the French of R. F. H. Laennec, M. D. by John Forbes, M. D.—p. 208.

[2] Haller's Not. in Prælect. Boerh. D. C. vi.—Haller's Opuscula Anatomica de Resp.   Gott. 1751, p. 91 and 345.—Marherr's Prælect. in Boer. Inst. vol. iii. p. 391.

[3] Haller's Opuscula Anat. p. 44.

[4] Philos. Trans. for 1809.

[5] Vide Morgagni de Sed. et Caus. Morb. Ep. v. ; and Trans. of a society for the improvement of Med. and Chir. Knowledge, vol. i. in which an interesting " Case of Emphysema, not proceeding from local injury," is given by Dr. Baillie.

Notwithstanding the experiments detailed by Sir Everard Home, in his Croonian Lecture, published in the Phil. Trans. for 1818, I am induced to believe that the gas in question is azote, rather than carbonic acid ;—because the alkali in the blood is not saturated with carbonic acid ; because the serum of the blood is capable of absorbing carbonic acid gas, rather more even than water, as I have ascertained by experiment ; because, during the coagulation of blood spontaneously, and the coagulation of serum by heat, I have never observed carbonic acid gas to be disengaged, when the experiments were properly made in vessels to which air could not have access, as in tubes completely filled with blood or serum, and inverted in blood or mercury ; and lastly, because I have not been able to procure carbonic acid gas from blood

It has been asserted lately, that air thus found is, in every instance, the consequence of putrefaction. But surely the accurate Morgagni was not so egregiously deceived. Many times, I have noticed air in the vessels of the pia mater, in bodies only a few hours dead ; and very lately I detected some in the internal jugular vein of a body that had been dead only 18 hours, and free from every mark of incipient putrefaction ; and I lay the more stress on this observation, because it was very carefully made, before any large vessel was divided through which air could gain admission. Further, air seems to pass pretty readily from the air-cells of the lungs into the pleura. Is not this proved by an experiment of Hales ?[1] And an experiment which I have made, and which I may briefly notice, seems to afford some proof of it. Immediately after death, before the muscles had lost their irritability, I inflated the lung of a dog under water, by means of a double bellows, through the trachea. Air, in exceedingly minute bubbles, escaped from the surface of the pleura covering one of the inferior lobes; and, on making gentle pressure with the fingers on any part of the inflated viscus, the same appearance presented itself.

These circumstances, which I have ventured to bring forward as somewhat favourable to the idea of the secretion or exhalation of azote, are still far from conclusive. After having given the subject all the attention in my power, I do not venture to draw a positive conclusion. I have thought it right to state what I have observed relative to a topic so interesting and obscure, and to notice such facts as seemed to bear more immediately on the question, in hope of exciting further inquiry, by which alone the true source of the azote apparently evolved in the preceding instance can be ascertained.

The effect of nitrous gas introduced into the pleura now requires consideration. I have made several trials farther with this gas. When admitted nearly pure into the pleura, it produced very serious symptoms; but did not prove fatal, provided the lung on the opposite side was free to act. The distressing symptoms usually subsided in about twelve hours, and then, on killing the animal, the greater part of the nitrous gas was found to be absorbed ; the pleura was free from inflammation, the substance of the lungs very slightly inflamed, and the blood exhibited a brownish hue. From these circumstances it may be conjectured that nitrous gas produces its deleterious effects, after it has been absorbed, either by acting on the blood immediately, or on the air-cells of the lungs and the blood

just drawn from the vessels, and still warm, when placed under a receiver and completely exhausted of air. I may here remark, that I have made two experiments on blood in vacuo, and in both with the same negative results. In one instance the arterial blood of an ox was employed ; in the other, the blood of a man in health. In the former, eight ounces were used ; in the latter, one ounce. In both instances, the blood remained perfectly tranquil, when the vacuum was as complete as could be made with a good air-pump, and of course did not exhibit the slightest traces of the disengagement of any air.

[1] Vide Stat. Essays, i. 252.

conjointly, when converted into nitrous acid in the course of the pulmonary circulation.

On the admission of nitrous oxide into the pleura, I have made one experiment only. About 30 cubic inches of this gas, contaminated with 25 per cent. common air, were passed into the pleura of a dog. The animal exhibited no uneasy feeling, and immediately after appeared to be rather exhilarated. It continued apparently in good health for 24 hours, when it was killed. Five cubic inches of air were procured from the pleura, which consisted of 10 per cent. oxygen, and 90 azote, being quite deprived of nitrous oxide. The pleura and lung exhibited no unusual appearances which could be referred to the gas absorbed.

Mr. Abernethy, in his ingenious Essay on the Functions of the Skin, has proved that that texture is possessed of a power of absorbing and exhaling certain gases, which it exercises according to laws peculiar to the animal economy.[1] The preceding experiments seem to show that the pleura is possessed of a similar power in respect to absorption, and that in exerting this power, like the skin, it acts with greater energy and effect on one gas than on another. Whether the analogy will hold good as regards exhalation also, must be decided by future inquiry.[2]

Reflecting on the preceding experiments on the absorption of different kinds of air introduced into the pleura, it appeared probable that mucous membrane, like serous membrane and the skin, may possess the power of absorbing air. In relation to this view, I thought it worth while to examine the air contained in the antrum maxillare, and in the frontal sinus. I chose for the experiment the head of the sheep, in which these cavities are large, the openings by which they communicate with the atmosphere small, and the membrane by which they are covered an active secreting surface. I collected the air by perforating the cavities under water, about fifteen minutes after the death of the animal. In two different instances, the results of the examination of the air were the following: the air from the antrum maxillare in one instance consisted of

4·3 carbonic acid,
13·0 oxygen,
82·7 azote.

From the frontal sinus, of
13·5 oxygen,
86·5 azote,

[1] Surgical and Physiological Essays, Part 2, by John Abernethy.
[2] When the experiments were made, which are detailed in the preceding pages, I was not acquainted with the ingenious and elaborate researches of M. Nysten on the injection of different kinds of gas into the blood-vessels and into the pleura, as described in his excellent work, "Recherches de Physiologie et de Chimie Pathologiques," published in 1817. The results I have obtained in most respects agree with his. Had he examined the residual air, and ascertained its composition, after the death of the animal experimented upon, the value of his researches, great as it is already, would have been much enhanced.

without any carbonic acid gas, the absence of which may have been owing to there being a good deal of mucus in the cavity ; the mucus might have absorbed it.

In another instance the air from the antrum maxillare consisted of

> 4·2 carbonic acid,
> 13·8 oxygen,
> 82·0 azote.

From the frontal sinus, of

> 4·5 carbonic acid,
> 9·5 oxygen,
> 86·0 azote.

On the supposition that the air, previous to its entering these cavities, had undergone a partial change from respiration, the results described seem to indicate an absorption of oxygen.

Other facts might be adduced, which, like the preceding, though not conclusive, tend to support the idea, that mucous membranes are capable of absorbing air.   Of this kind, I conceive, are the results of the experiments of M M. Magendie and Chevreul, on the composition of the air contained in the human stomach and intestines ;[1] and very recently I have met with a fact, the bearing of which appears to me similar.   In examining the body of a soldier, who had died of complicated disease, I found the cœcum exceedingly distended with air, and of a bright red colour, as if highly inflamed, whilst the ascending colon was unusually contracted.   The air collected under water amounted to thirty-six cubic inches, and consisted of 11 carbonic acid, or air absorbable by lime-water, and of 89 chiefly azote, judging from its extinguishing flame, and not being itself inflammable.   I had not the means of ascertaining if any traces of carburetted hydrogen were present.

The question, whether mucous membranes are capable of absorbing gases, I need not say, is one of great importance in relation to the theory of respiration, and on that account deserving of particular attention.   The theory which in 1823 was most generally adopted, is recommended by its simplicity, but is not well supported by the analogies and facts of physiology, which seem to favour the doctrine of the absorption of oxygen into the blood, and the evolution of carbonic acid; and *that* perhaps, not in the air-cells of the lungs alone, but likewise along the whole tract of the primæ viæ, and over the whole of the external surface of the body.[2]

[1] Ann. de Chim. et Phys. ii. 292.
[2] Many facts which, since the above was written, have been brought to light, especially by the researches of Dr. Edwards, might be adduced in corroboration of the remark in the text.   In the winter of 1838, I examined the series of cells, filled with air, which occur in that beautiful osseous structure, the vertebral column of the wild swan.   The contained air, I found composed of

> 83·3 azote,
> 16·7 oxygen.

CASE II.—Patrick Calnon, of the 50th regiment, was admitted into the General Hospital at Fort Pitt, on the 9th of May, 1823, on his return from Jamaica, from whence he was sent home invalided on account of hæmoptysis, attributed to a severe fall on the left side of the chest eighteen months previously.   Before the accident, his health had been uninterruptedly good.

Until the 13th of May, his complaint exhibited nothing peculiar. Early on the morning of that day, after a violent fit of coughing, the symptoms of pneumathorax began to appear ; and they continued to increase until the 21st.   The most prominent symptoms were, a feeling of extreme tightness about the chest and abdomen ; rapid and difficult respiration, between 30 and 40 in a minute ; great anxiety of countenance and agitation of mind, accompanied with a small pulse of 130, cold sweat on the face and neck, and much prostration of strength.   On examining the chest, the left side was found more protuberant, and in all its dimensions larger than the right. It was tense, and on percussion sounded remarkably hollow and tympanitic.   The heart was felt beating on the right side, even under the mamilla.

Satisfied that the distention, and the distressing symptoms accompanying it, resulted from air in the pleura,—reflecting on the suddenness of the attack, the tolerable health the patient was in previously, and the now imminent danger of life,—I proposed in consultation the operation of paracentesis,—which was approved of, and, with the ready assent of the suffering patient, immediately performed.

With a small trochar, attached to a flaccid bladder, I carefully perforated the left side of the chest, between the 8th and 9th rib, having previously divided the integuments with a scalpel.   On withdrawing the stilette, a little air rushed out and was collected in the bladder, but not in the quantity that I expected ; it did not exceed 5 cubic inches.   On examination it was found to consist of azote, and a little carbonic acid.

Conceiving that the operation had failed in consequence of adhesions in the part of the pleura punctured, and encouraged by the composition of the air collected, and the slight relief which the patient experienced, a repetition of the operation was decided on, and performed the next day.

The chest now was perforated, just below the left papilla.   On withdrawing the stilette into the bladder, a large quantity of air rushed out and distended the bladder ; and on separating the bladder from the canula (by cutting it off, after having secured the air in the former by a tight ligature) air from the chest, for several seconds

The examination was made two days after the bird had been shot : probably during life, the proportion of azote is greater.   Probably, wherever air penetrates in birds, whether into the abdominal air-cavities, or the osseous ones, there is a continued absorption of oxygen going on, giving rise to several effects connected with the economy of this class of animals, as buoyancy, high aeration of blood, and high animal temperature.

continued to rush out with violence. When the rush of air had ceased, and it was found that air began to pass in on inspiration, the canula was withdrawn, and the wound was closed by adhesive plaster.

The patient experienced sudden and great relief, exceeding his power to express.[1]

For about a month after the operation, the patient continued progressively to improve. On the 17th of June, he was as well as when first admitted into hospital. His appetite was good, and his cough little troublesome. He could lie on the left side, which he was unable to do many months prior to the operation. Both wounds were healed. The left side of the chest was diminished considerably in volume, and was much less tense and tympanitic. The heart, however, was still felt pulsating on the right side, and a fluctuation of fluid in air could be heard in the left side, on shaking, or suddenly moving the body.

The following week the symptoms were less favourable, and those of hydrothorax especially were more severe. When the patient attempted to lie on the right side, he was instantly seized with a fit of coughing; and when the body was shaken, the sound of fluid fluctuating in air in the left pleura was distinctly audible even at the distance of several yards.

As the patient's health was pretty good, it was deemed advisable to repeat the operation of paracentesis and draw off the fluid, before the case became desperate.

Having, in cases of empyema, found some inconveniences attending the puncturing of the pleura, between an intercostal space, I was induced, in this instance, to follow the method described by Hippocrates, of perforating one of the ribs.[2] The fifth rib was selected ; and having cut down upon it with a scalpel, just below the papilla of the breast, I bored through its substance with a carpenter's auger, and punctured the pleura with a small trochar, as nearly as possible of the same size as the auger. On withdrawing the stilette, it was followed by a stream of transparent fluid, fourteen ounces of which were allowed to escape ; and, then, leaving the canula in the perforation, it was closed with a cork, and secured by proper dressings.

Daily, during six weeks, more or less fluid was discharged through the opening in the rib, amounting, altogether, to more than twenty pints. At first, the fluid was transparent, of specific gravity 1021 ; it coagulated when heated, and contained some alkali in the state of sub-carbonate. In a few days pus appeared in it, and gradually increased in quantity until the 15th of July, when the discharge was almost entirely purulent; after which time, the proportion of pus and serum varied considerably,—one sometimes predominating,—

[1] The air collected in the bladder amounted to 25 cubic inches. Examined by means of lime-water and phosphorus, it was found to consist of 93 azote, and 7 carbonic acid ; it had no offensive odour, or odour of any kind.

[2] Hippocrat. de intern. adfect. cap. xxiii.

sometimes the other.   It may be deserving of notice, that at no time was I able to detect free carbonic acid in the fluid discharged.

Although, from the sound of fluctuation in the chest, air was evidently contained in the pleura, yet none escaped with the fluid during the first fortnight;—afterwards a considerable quantity was daily expelled.   By means of the perforation in the rib, and using a flaccid bladder, furnished with a trochar, I could, at any time, collect this air for examination, with perfect ease.

The first portion collected (about twenty cubic inches) was on the 15th of July;—examined by means of lime-water and phosphorus, it was found to consist of

> 7·5 carbonic acid,
> 2·5 oxygen,
> 90·0 azote.

The second portion collected (about thirty-five cubic inches) was on the 20th July; it consisted of

> 6·0 carbonic acid,
> 5·5 oxygen,
> 88·5 azote.

The last portion collected (about forty cubic inches) was on the 29th of the same month; it consisted of

> 8·0 carbonic acid,
> 4·0 oxygen,
> 88·0 azote.

For some days after the operation of paracentesis, the health of the patient deteriorated; his appetite diminished; and he had feverish symptoms, but unattended with rigors.   At the time, judging from the increasing proportion of pus which appeared in the fluid discharged, the pleura was probably undergoing inflammation, though the side affected was quite free from pain.   Gradually these symptoms subsided; and, on the 15th July, he felt better than before the operation; he had less difficulty of breathing,—very little cough, and he could lie on either side.   His improvement was progressive until the 23d July;—afterwards his health again became worse, his appetite impaired and his spirits low; he had slight pyrexia, and a feeble pulse, varying in frequency from 90 to 120.   This unfavourable change was attended with the emission of a large quantity of air from the pleura, and with an alteration in the character of the fluid discharged,—which had become more purulent, of a greenish hue, and of an offensive odour.   The patient expired suddenly on the 29th July.

The body was examined twelve hours after death.   The cavity of the abdomen having been laid open, the diaphragm was found slightly protruding into the left hypochondrium, without displacement of any of the abdominal viscera.

The body having been immersed in a bath,—on puncturing the left pleura, between the first and second rib, air to the amount of

one hundred and seventy cubic inches issued out, and was collected: it consisted of

> 16·0 carbonic acid,
> 1·5 oxygen,
> 82·5 azote.

The left pleura contained besides about six ounces of pus,—so much having subsided from the water, which entered the chest to supply the place of the air.

The right pleura was quite free from disease. The right lung appeared to be healthy,—but, on minute examination, numerous granular translucent tubercles were detected, disseminated through its substance, and two small vomicæ were found in the upper part of its superior lobe. The heart was displaced: it was situated on the spine, inclining a little to the right side, and the position of the greater part of the œsophagus was similar. The pericardium was firmly attached to the middle lobe of the right lung by a firm adventitious band. On exposing to view the left cavity of the chest, the inside of the pleura exhibited a surface of milk-white granular coagulable lymph, about two lines thick, equally diffused on the costal and the pulmonic side. Excepting the cicatrices externally in the skin, no traces could be detected of the first two operations, nor was there any mark of the last operation, exclusive of the small opening that had been carefully kept open, just large enough to admit the trochar, which had been daily introduced to draw off the fluid and allow the air to escape. On maceration of the rib, it may be remarked, a very narrow ring of bone was found exfoliating from the perforated part. The left lung was very much condensed, and so firmly confined by its thickened pleura, that it did not dilate when air was driven into it with some force by a double bellows attached to the trachea. This experiment was made with the lung under water, for the purpose of ascertaining, if any, and what kind, of communication, existed between the lung and the pleura, with a view to discover the origin of the air accumulated in this cavity. Two communications were thus detected; one in the inferior, the other in the superior lobe: the former was so exceedingly small that it could not be traced. The latter opening was sufficiently large to admit a surgeon's probe,— and its course was easily followed. It was found to communicate obliquely with a ruptured opening in the side of a large bronchial tube, situated immediately under the pleura. No cavity could be detected there. The adjoining pulmonary substance appeared to be condensed from compression: the substance of the lung generally was in the same state; it contained a few minute tubercles; and no other traces of disease.

In the fatal case of Iredill, before described, pneumathorax took place in consequence of ulceration effecting a communication between the cavity of the pleura and a vomica in the lung. In this instance, the disease originated without the intervention of a

vomica,—but, whether without ulceration, connected with the soft-
ening of a minute tubercle, is doubtful.   When I first considered
the case, I was disposed to attribute the communication to the rup-
ture of a superficial bronchial tube, and to express surprise that
accidents of this kind are not more common, taking into account
the large number of bronchial tubes which lie immediately under
the pleural covering of the lung,—the delicacy of the membrane,
and the facility with which it and the bronchia are ruptured.   The
accuracy of this view appears to me now very questionable, inas-
much, as the delicate pleura has, in its healthy state, always the
support of the parietes of the chest, and being constantly in contact
with the costal pleura, the risk of its rupture may be supposed to
be exceedingly small.

In a professional point of view, it might be an interesting though
not an easy task, to trace the different steps of the disease, of which
I have given a brief history, from its commencement to its termina-
tion, and connect the symptoms with the organic changes which
occurred.   As more appropriate to this place, I shall confine the
few remarks I have farther to make, to the air procured from the
pleura.   The following table exhibits, at one view, the composition
of the air collected from the chest at different times.

When Collected.	COMPOSITION.		
	Carbonic Acid.	Oxygen.	Azote.
May 21	7	-	93
July 15	7·5	2·5	93
,, 20	6	5·5	88·5
,, 29	8	4	88
,, 30	16	1·5	82·5

To what were these variations in composition, which the table
exhibits, owing?   I cannot conceive that they depended entirely
on the admission of variable quantities of atmospheric air by the
external opening; because exceedingly little atmospheric air could
enter through that channel, both from the great care taken to ex-
clude it, and from the valvular nature of the passage.[1]   I believe
we must look chiefly to the source of the air, and the absorbent
power of the pleura for the explanation in question.

In this case, as in Iredill's, there is reason to suppose that the air
accumulated in the chest was common air, more or less vitiated by
respiration previously, and more or less altered by the process of
absorption after entering the cavity of the pleura.   Taking this
view of the subject, the composition of the air, each time it wa

[1] The perforation being slightly oblique, the pleura costalis lined with
coagulable lymph, closed the external aperture in the rib on expiration, and
prevented completely the egress of air, even when the dressings were re-
moved and the external aperture uncovered.   When the trochar was intro-
duced, the stilette was withdrawn during expiration, and the finger was ap-
plied to the mouth of the canula during each inspiration.

examined, is easily accounted for, excepting in the last instance,
when the proportion of carbonic acid gas was found to be so large,
after death.   On what this depended it is not easy to say; it is
matter for conjecture, and seems to require further investigation.
Messrs. Allen and Pepys, after a forced expiration, found air from
the lungs to contain as much as 9·5 per cent. carbonic acid gas;[1]
and in the different instances that I have examined the air con-
tained in the lungs a few hours after death, I have found the pro-
portion of carbonic acid gas to vary from eight to twelve per cent.;
thus, in a fatal case of empyema, the air procured from one lung,
which was sound, consisted of

> 8·3 carbonic acid,
> 5·0 oxygen,
> 86·7 azote:

that from the other lung, which was in part condensed, and as it
were hepatised, of

> 12·5 carbonic acid,
> 2·0 oxygen,
> 85·5 azote:

whilst, in another case, in which one lung was sound, and the
other abounded in minute cavities full of pus—air from the sound
lung consisted of

> 12·2 carbonic acid,
> 3·0 oxygen,
> 84·8 azote.

Had the proportion of carbonic acid gas, in the instance under
consideration, been within these limits, the explanation would have
been attended with little difficulty; but, exceeding these limits, one
is almost disposed to refer it to exhalation or secretion from the
pleura.   And if, at the same time, we suppose, that a similar ex-
halation took place into the bronchia, it might account for the sud-
den death.

In support of this idea of the secretion or exhalation of air into
closed cavities, in addition to what has been already stated, I may
notice the two following instances, which have since come under
my observation.

On the 23d of May, 1823, on examining the body of a soldier,
who had died of chronic dysentery, complicated with an ulcer in
the larynx, the cellular membrane in both mediastina was found
vesicular and distended with air.   The vesicles were burst under
water, and a half cubic inch of air was collected, which was found
to consist of

> 7 oxygen,
> 4 carbonic acid,
> 89 azote.

[1] Phil. Trans. for 1808.

The surrounding parts were carefully examined ; particularly the trachea, lungs and œsophagus, but no passage could be detected through which air could have entered the mediastina ; nor could any air be forced into them by distending the lungs by means of a double bellows.   It may be, that the oxygen found in the mixed airs was extraneous, and derived partly from common air adhering to the surface of the cellular membrane, and partly from air of the atmosphere penetrating through the delicate vesicles during the preparatory dissection, when they were exposed to its influence, for half an hour at least.

On the 2d of June of the same year, in examining the body of a soldier, aged 36, who had died of tubercular consumption, I found air-vesicles on the surface of the lungs, similar to those described by Dr. Baillie in his Morbid Anatomy ;[1] and considered by him as resulting from the secretion of air, and not from the extravasation of air under the pleura, agreeably to the opinion advanced by Laennec.[2] The air contained in the vesicles in this instance, consisted of 5 parts of azote, and 1 part of carbonic acid.   The quantity of air collected and examined, did not exceed 1-20th of a cubic inch ; I could not detect in it any traces of oxygen.   In neither case, it may be remarked, were there any signs of incipient putrefaction ; the weather was cool; and the examination was made a few hours after death.

XXV.

ON THE EFFECTS OF THE SUN'S RAYS ON THE SKIN.

It is known to every one, that exposure to the sun's rays renders the skin brown ; but I am not aware that this well known effect has hitherto been investigated with any minuteness, if at all, either in relation to the manner in which it is produced, or the part of the skin in which it takes place, or its exact cause, or its consequences.

The following observations were made with the desire of elucidating these points: they were collected whilst I was stationed in the Ionian Islands in 1826-27, and were originally published in the 3d vol. of the Transactions of the Medico-Chirurgical Society of Edinburgh.

1. *Of the changes connected with the discolouring effect of the Sun's rays.*

For the purpose of ascertaining these changes, a portion of the back of the fore-arm, which had never before felt the sun's action,

[1] Fifth Edition, p. 80.
[2] Treatise on Diseases of the Chest, &c. p. 89.

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was exposed to bright sunshine, in Corfu, during an hour and a half, on the 29th July, 1826, in the middle of the day, when the thermometer was at 78° in the shade.   At the end of that time the skin was slightly painful, red, and hot.   On the 1st August the erythema was nearly in the same state ; during the night the redness of the skin had been heightened, and the sensation of pain increased.   On the 2d, there was very little alteration ; a thermometer applied to the inflamed part rose to 96°, or 1° higher than when applied to the adjoining skin.   On the 3d, desquamation had commenced at the circumference ; there, where the cuticle had separated, the part was brownish-red, and not painful.   In the middle, where the cuticle firmly adhered, the colour continued to be rose-red, and the pain continued, though in a less degree.   This middle part, it may be remarked, in which the erythema was most durable, was most inflamed, the sun's rays having struck on it perpendicularly ; whilst, on the circumference, from the rotundity of the arm, they impinged on it obliquely.   On the 5th, desquamation was making progress ; pain had ceased ; the part was reddish-brown at the edges, but still red at the centre ; the temperature of the part was not above that of the adjoining skin.   On the 8th, the part was uniformly of a reddish-brown ; desquamation was still taking place, the new cuticle separating almost as fast as it formed, not in continuous pieces, as in the first instance, when the old was detached, but in small scales.   On the 18th, the part was of a light brown, with a very slight admixture of red, and its tendency to desquamation was very little greater than natural ; in brief, it was in that state in which the skin is commonly said to be when tanned by the sun.

2. *Of the part of the Skin in which the discoloration takes place.*

Dr. Bostock, in his " Elements of Physiology," remarks, "It has not been ascertained upon which part of the integuments the sun acts, whether upon the epidermis, the corpus mucosum, or the cutis;" and he immediately adds, "but it is probably upon the epidermis, because we are informed that the tan of the skin is removed by blisters."

Were it a fact that the skin is rendered fair by blisters, the argument would be plausible,—I had almost said conclusive ; but, as it is well known that blisters themselves render the skin brown, this argument can hardly be received.

From the observations which I have made, and from analogical reasoning, I am disposed to believe that the discoloration takes place beneath the cuticle, and that the seat of it principally is the surface of the cutis.

1st. As the sun's rays bleach hair, and as there is a considerable analogy between the hair and the epidermis, its effect on the latter, it might be expected, would be similar.[1]

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[1] It is worthy of remark that the effect of the sun's action on the human

2dly. I have carefully examined the cuticle detached in conse-
quence of inflammation from insolation, and I have not found it
tanned in the slightest degree.

3dly. Are not the phenomena described in the preceding section,
relative to the immediate effects and consequences of exposure to
the sun's rays, almost sufficient to convince one that the cutis is the
true seat of the discoloration ?   Were the epidermis the seat of it, it
ought to be immediately discoloured by the sun's rays, which it is
not ; and when the epidermis separates, the skin should be fair ; but
the reverse of this is the case,—not till it separates does the skin lose
its bright rose-red hue; and not till after several successive desqua-
mations is the tan of the skin well impressed and established, and
many months elapse before it disappears.[1]

4thly. I have examined, with some attention, the cuticle of the
negro, of people of colour, and of Europeans who have become
dark-brown from exposure to the sun's rays within the tropics.   In
each instance, when detached, it has appeared much less coloured
than the skin ; and, when minutely inspected, it has been found to
owe its colour to colouring matter attached to it, detached from the
cutis.

Lastly, I have preferred assigning the surface of the cutis as the
seat of the discoloration, (supposing it to be proved that the cuticle
is not,) rather than the rete mucosum or corpus mucosum of authors,
as the very existence of such a texture is problematical.   From the
experiments which I have made on moles, and the coloured areola
of the mamilla of fair persons, and on the skin of the negro, I am
disposed to believe that the colour, in all these instances, is owing
to a colouring matter deposited in minute particles, or filaments, on
the surface of the cutis, as a secretion analogous, in its chemical
properties, to the pigmentum nigrum of the eye.[2]   In the skin of the

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hair is quite different from its effect on the skin,—bleaching the one, as much
as it darkens the other ; tending to bring both to a state, as regards hue, best
adapted, probably, for defence, such as occur in the Arabian horse, the finest
breed of which have a white coat on a black ground, they are in fact negro
horses with white hair.   Every observant traveller must have witnessed in-
stances in illustration, of what has just been observed.   The most striking
example of the kind which I recollect, is afforded by the female inhabitants
of the border town of Itri, on the confines of the Neapolitan and Roman
states, where the women are in the habit of exposing themselves, their heads
uncovered in the open air ; and in consequence there is a striking contrast
between the dark sun-burnt countenances, and their comparatively light hair,
which, variously discolored by the action of the sun's rays, and generally
much neglected, imparts to them an appearance of extreme wildness.

[1] The discoloration produced in August, by exposure of the skin for an
hour and a half, at the expiration of seventeen months, was just visible.   I
may add, that I have found it to continue much longer on a part always
covered, as the arm, than on the back of the hand, which has been covered
only in the open air.

[2] I find that the colouring matter of the skin of the negro, and the pigmen-
tum nigrum of the eye, are acted on very similarly by the three mineral acids,
and a solution of potash, when heated, and by the sulphurous acid.   By the

white, even in the parts discoloured, as in the areola already mentioned, I have not been able to discover any traces of a corpus mucosum, when the cuticle has been separated by means of immersion in the sulphurous acid. I have found the brown colouring matter, as before noticed, impregnating the surface of the cutis, and to be separated with difficulty by scraping it. In the case of the negro, the colouring matter is deposited more thickly, and more in the form of a membrane; yet I have not been able to detach it as a membrane; and only in very minute portions, and that by scraping when the cuticle has been raised and separated with as little inflammation as possible. The evidence in favour of the existence of a corpus mucosum, obtained either by maceration of the integuments, or by the application of blisters, appears to be very doubtful. By the first process, a gelatinous or mucus-like surface may be formed: by the second, a false membrane may be produced by the effusion of coagulable lymph, exactly resembling a corpus mucosum. I do not make these remarks hypothetically, but from experience,—from observing the effects of blisters on parts of the skin in which there have been moles; on the areola of the mammilla; and on the skin of the negro. In all these examples, the effects generally are very similar. If the blister is mild, the cuticle is simply raised; in the instance of the negro, with a very little colouring matter adhering to it. When severe and long continued, not only is the cuticle raised by serum effused, but also by coagulable lymph, to which is attached colouring matter, and which may easily be mistaken for a coloured rete mucosum, and which is easily separated as a continuous membrane. When severe inflammation and suppuration is excited, the colouring matter either comes away spontaneously, or is most easily detached.[1] It appears to be most firmly connected with the cutis in the instance of moles, next in that of the brown areola of the nipple, and least in that of the black skin of the negro. The part, in healing, when covered with the first formed cuticle, is red; it soon becomes brownish, but a considerable time elapses before it acquires its former intensity of colour. In the instance of the negro, in which I have watched its progress, the secretion of colouring matter began at the edges, and spread towards the centre; and then, after a few days, spots of black appeared in the middle, which enlarged till the whole area was coloured. When a part not discoloured is blistered, in healing, it passes from red to brown; and it is often a long time before the part regains its healthy hue; generally, I believe, the fairer the skin, the less it is made brown by a blister,

former both are dissolved; by the sulphurous acid, they are rendered of a light brown colour. They are not dissolved by these acids, or by the alkali, when cold; nor is their colour changed by a solution of chlorine in water,—contrary to what is commonly asserted of the colouring matter of the skin of the negro. Both bear a high degree of temperature, apparently without change, viz. that nearly of a dull red heat.

[1] Thus it may be obtained in large pieces, very much resembling a membrane; but the connecting medium, I suspect, is coagulable lymph.

and the sooner it recovers its original whiteness ; and I believe the hotter the climate and season, so much the more slowly it regains it.

3.—*Of the cause of the change of colour, and of the manner in which it operates.*

My experiments relative to the cause of the change of colour produced in the skin by the sun's rays, are not so satisfactory as I could wish. They tend, however, rather to prove that the effect is produced solely by the undecompounded rays. I have exposed, for more than two hours, and that repeatedly, the delicate skin of the under part of the fore-arm to the solar spectrum; and I have concentrated the differently coloured rays of the spectrum, by means of a lens, on the skin, but without occasioning either erythema or discoloration.

Relative to the manner in which the effect is produced, is it immediate and direct, or mediate and indirect? In other words, is it the simple effect of the sun's rays impinging on the skin, or the effect of the inflammation which they occasion; or do the sun's rays act in both ways?

That they act powerfully indirectly in producing discoloration, by exciting inflammation, the facts already mentioned are, it appears to me, sufficient proof. Indeed, whatever cause excites inflammation or irritation of the skin, seems to have an analogous effect, and to discolour it. Erysipelas, erythema, most of the exanthemata, burns, ulcers, excoriations, all occasion this effect, (and I believe cold even is not an exception,[1]) in different degrees, and very much in proportion to the intensity of the preceding inflammation, but whether exactly in that ratio it is difficult to determine. And we witness something of the same kind in mucous membranes; at least, I have observed that the cicatrices of old ulcers of the intestines are always discoloured, and either gray, blue, or almost black, apparently according to the degree of severity of the local disease which preceded them.

I have just said it is difficult to determine if the effect of discoloration be exactly proportioned to the inflammatory effect. There are circumstances in favour of its not being so. The erythema produced by strong acetic acid, and the vesication occasioned by the leaf of the common walnut tree, are followed by discoloration unusually dark and durable. Nor are there facts wanting

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[1] Is not the dark colour of the inhabitants of the arctic regions as much owing to the inflammatory or irritating effect of the extreme cold of winter as to the scorching influence of the continued sunshine of summer?

Milton's highly poetical expression, the burning "frore," which he uses in his description of the damned regions, is in accordance with accurate observation of the agency of intense cold, in many respects so analogous to that of great heat:

> "The parching air
> Burns frore, and cold performs the effect of fire."

which indicate that the change of colour may take place without
inflammation, and go on increasing in intensity gradually, from
continued exposure to the sun, or even too bright light, without in-
flammation having been once produced.   I remember an instance
demonstrative of this, in the person of an excellent and most ami-
able young officer (now no more), a case of tubercular phthisis,
complicated with other organic disease, who, in hope of deriving
benefit from sailing, and sea air, was taken from his room, where
he had been confined many months, and conveyed on board ship,
where he was placed under a convenient covering constructed on
deck, sheltered always from the direct rays of the sun, but exposed
to the bright light of the summer sky of the Mediterranean.   In a
short time, thus situated, he lost the pallid hue of the sick-chamber,
and became almost as deeply tanned as a native of southern Europe;
and I was particular in ascertaining that the change had not been
preceded by the slightest erythema, or any sensible desquamation.
I may mention, in confirmation, the result of exposing, a second
time, to the sun's rays, the part tanned, as in the experiment first
related.

On the 18th August, the part first acted on was exposed for two
hours, between 10 and 12 o'clock, when the sky was unclouded,
and the temperature, in the shade, about 80°.   Immediately after
this exposure, the tanned part was browner than before, and the
adjoining white part, now exposed for the first time, was slightly
red. On the 19th, the tanned part was distinctly brown and redder,
a very little warmer than natural, and very slightly tender.   The
adjoining part was florid red, slightly painful, and hot.   On the
23d, the tanned part was merely brown, a shade darker than before,
while the adjoining part was undergoing desquamation, and begin-
ning to lose its vivid inflammatory hue.   And, farther, in confirma-
tion, I may relate, that I have been at some pains to learn from
natives of the Ionian Islands, especially of the lower classes, who
are very much in the open air, what effect they have experienced
from the sun's action.   The result of my inquiry is, that very few
of them have ever experienced the blistering or scorching effect of
the sun; and when they have experienced it, it has commonly been
on a part of the body not accustomed to be exposed to light; and
on some occasion of unusual exposure, as that of bathing in the
open sea.   From all which, may it not be inferred, that the sun
acts both indirectly, by the medium of inflammation, in changing
the colour of the skin, and directly, without the intervention of in-
flammation, in producing the same effect, or in heightening it when
produced?

Lastly,— Of the consequences of the discoloration of the skin.

Sir Everard Home, in an interesting paper published in the Phi-
losophical Transactions for 1821, has proved that, when the skin
is painted black, it is defended from the scorching effect of the sun's

rays; and, he thence infers, that the dark rete mucosum of the negro possesses the same protecting power.

I have made experiments similar to those of Sir Everard Home, and have modified them, and all of them with the same results. All the opaque colours of which I made trial, applied to the skin, whether red, orange, blue, or green, have afforded protection from the scorching influence of the sun's rays, equal to that afforded by black.[1]

But though I have confirmed the experimental results of Sir Everard Home, it appeared to me, when reflecting on the subject, that his inference was not so well established as at first view might be conceived. It is founded on analogy, and that analogy not perfect; there being this difference between the skin of a white person painted, and of a negro with a black skin, that, in the one instance, the black surface is laid on the semi-transparent cuticle, whilst, in the other, it is situated under the cuticle, and on the surface of the cutis. In the one instance, the extinguishing medium is external to the insensible covering of the body; in the other, it is in contact with the sensitive surface, and may be considered as a part of it. Circumstances, too, relative to the very great penetrating power of the sun's rays, have had rather a similar tendency to augment my doubts of the strict accuracy of this analogical conclusion. As the facts which I now allude to appear to me to be new and curious, I shall mention some of them. When the sun's rays are concentrated by a lens, they penetrate, I find, through bone, as a portion of the cranium,[2] through nine folds of black crape; and, what is more extraordinary, through rolled platinum. It was easy to ascertain their penetrating through the former substances, by a luminous point appearing on a surface beneath; but through the opaque platinum no light passed, yet the rays of heat passed, which was best indicated by the sensation produced when the metal was placed on the sensitive skin, the only part of which affected was that corresponding to the focus of the lens, the metal itself not becoming sensibly warmer.[3] Taking, then, into consideration the difference between the painted cuticle and the dark rete mucosum, and this very remarkable penetrating power of the sun's rays, it appeared to me that more direct experiments than those of Sir Everard Home were requisite, to ascertain, beyond all doubt, if the function of the colouring matter of the skin of the negro is really such as it has been inferred to be. With a view to this, I have subjected the skin of the negro to the direct rays of the sun, and I have made a similar trial on a mole on a fair skin.

[1] Considering the effect mentioned above, the usage of the ancient Britons in painting their bodies may be referred to a purpose of utility as well as of barbarous ornament,—the paint with which they bedaubed themselves must have answered, in part, in place of clothing.

[2] This circumstance may help to explain the effect of the sun on the brain in producing the malady commonly called *coup de soleil*.

[3] No effect was produced through the platinum on moist chloride of silver.

After two hours' exposure to the sun, its rays moderately concentrated by a lens, (for the experiment was made in winter when the temperature was between 50° and 60°,) the part acted on, in which a dark brown mole was situated, became slightly red; the following day it was red, and just perceptibly painful; and about the fourth or fifth day, desquamation of the part commenced. The desquamation took place over the mole as well as the adjoining fair part, and the mole was evidently rendered of a darker colour. On the 27th December, when the sky was clear, and Fahrenheit's thermometer in the shade at 56°, a similar experiment was made on the fore-arm of a negro, and continued the same time. The skin acted on was a little hotter than the rest, just perceptibly darker, and it felt, he said, slightly sore. On the following day, the part appeared to be very little darker, and he said that it was slightly painful and swollen, but this last mentioned effect was not perceptible to my eye. On the 31st December, the pain had ceased; there was not the slightest appearance of desquamation, and it was only just perceptibly darker than the adjoining skin.

From these results, I am disposed to infer that the colouring matter of the skin of the negro affords some protection from the scorching effects of the sun's rays, but not complete protection, and that were his skin as much disposed to inflame from the action of the sun's rays as the skin of the fairest European, this colouring matter would not prevent occasional vesication. Some of the facts already mentioned tend to support this opinion, especially the circumstance that exemption from the scorching effect of the sun is not confined to the African negro, but is possessed equally by all the various races of men—the inhabitants of hot climates, who are much in the open air, and exposed to bright light, whether the colour be almost black, like that of the lower classes of Singalese, or of a dull straw colour, like that of the Bojesman of southern Africa, or of a ruddy brown, as in the instance of the Albanian shepherd of the mountains of Greece.

Nature, then, it may be remarked, is very provident, adapting the skin, impressed by the sun's rays, to bear them afterwards without inconvenience, or at least without painful suffering, the impression having apparently a protecting effect from farther annoyance, like the first attack of many of the infectious exanthemata; but with this difference, that the susceptibility to a renewal of the action is not long suspended, unless the cause is in constant activity. How long it is suspended it is difficult to determine: it is suspended in different degrees, probably in persons of different complexions and temperaments, least in the fair,[1] more in the brown European

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[1] Very fair persons, as regards complexion, are least affected by climate, and exposure to the bright light of a tropical sky;—in this respect resembling the albino. Unusual exposure to the sun's rays is painfully felt by them; they experience the sun-burn; but, after the erythema or superficial inflammation has subsided, and desquamation has taken place, the fairness of the skin is but little impaired,—and its liability but little diminished to suffer in the same manner from farther exposure.

races, and most of all in the deeply coloured Asiatic and African tribes.

Besides the foregoing, there are other means which nature em-ploys to counteract the influence of the sun's rays on the human body, and to keep down animal heat within the bounds of health, in situations where otherwise it would be most apt to be in excess, and mount to a feverish height. These means seem to be of two kinds, one external, the other internal.

Perspiration, both sensible and insensible, constitutes one of the first. It not only cools the surface in connection with evaporation, but also, when in the form of sweat, by dispersing the sun's rays, prevents or diminishes their scorching agency, as is easily shown by comparing the effect of the sun's rays, concentrated by a lens, on a dry and a wet skin. The internal means are not so obvious; but that they do exist, and act beneficially, seems undoubted. There is reason to infer, that the greater the atmospheric temperature is, the less oxygen is consumed in respiration—that the blood is less aërated, and there is less heat in consequence produced. Some facts bearing on this curious point have already been mentioned, when treating of animal temperature in summer and winter; and further on, some other facts tending to illustrate it will be brought forward, in an account of observations on the blood, in connection with respiration. Besides this, probably the principal balancing circumstance, others in addition may be pointed out by which na-ture fits man to bear with impunity, and with comparatively little inconvenience, the heat of the hottest regions of the globe of which he is a native. In a hot climate, especially within the tropics, the cuticle appears to be thinner than in a cold climate, so as to confine the animal heat less.[1] Moreover, I believe that the exhalants of the skin, and indeed the whole apparatus peculiar to this texture, is either more developed or more active, or both, under the influence of a high than of a low or moderate degree of temperature. And the condition of the blood seems to accord, as cold appears to in-crease its viscidity, heat its fluidity; consequently, *cæteris paribus*, in a hot climate, it will be less viscid than in a cold, more fluid and flow more freely through the cutaneous and subcutaneous vessels. Thus, by promoting perspiration, it will contribute to the cooling of the surface; and, being cooled itself, it will contribute again, when it flows back to the heart, to the reduction of the temperature of the internal parts.[2]

[1] The cuticle of the sole of the foot and of the palm of the hand is, under certain circumstances, an exception, as when exposed unprotected to the action of a hot surface, or subjected to much pressure. I have seen a negro sleeping with his feet so close to a fire, that the outer exposed surface was almost scalding to the hand applied to it, and yet, owing to the thickness of the part, to the individual it transmitted merely an agreeable warmth.

[2] I deduce this opinion of the blood being more liquid at a comparatively high temperature, as 88° or 98°, than at a low one, as 38° or 48°, from ex-periments on the blood, showing that a certain degree of cold thickens the blood, and that a certain degree of heat renders it more liquid, so that in one

Lastly, peculiarity of constitution appears to be intimately connected with climate, and the tolerance of different degrees of temperature with impunity. The African enjoys the best health, is in the highest spirits, and is capable of the greatest exertions in hot, moist regions, where the temperature is seldom below 80°; and is almost entirely exempt from those fevers of the intermittent and remittent type, which have been, and probably always will be, the scourge and destruction of Europeans in hot climates.[1] But, reverse the situation, and place the African negro where the European recovers his lost energy, shakes off the languor of the tropics, and is restored to health and strength,—there the former commonly droops, becomes languid, feeble, and diseased, and soon sinks into the grave.[2] And thus it is, no doubt, that each race is propagated and multiplied in the situation most suitable to the developement of its faculties and powers.

XXVI.

ON A PECULIARITY OF STRUCTURE OCCASIONALLY OCCURRING IN THE BASILAR ARTERY OF MAN.

Besides those peculiarities of structure of the basilar artery which are well known, there is one of not uncommon occurrence, which, to the best of my knowledge, has not hitherto been noticed by any anatomist. It is a band in the interior of the vessel, attached to its sides, and consequently intersecting it. It varies both in its dimensions and situation. I have most frequently found it near the junction of the vertebral arteries; very seldom near the commencement of the circle of Willis. Sometimes the band perfectly intersects the vessel; at other times only partially. Sometimes I have seen it not more than a line thick; occasionally two or three lines. Its appearance, as regards its nature, has always been similar and most analogous to a fibrous structure. In every instance, I apprehend, it may be considered as congenital, and not the effect of disease. This is in-

state it is better fitted for torpid hibernating animals, and those of a cold climate, and in the other, for animals in whom the functions of life are performed with energy, and that constantly, as in a warm climate.

[1] In regions more unwholesome to Europeans than the Maremma of Italy, as (to state from my own experience) in some parts of the interior of Ceylon, where not one European in a hundred escaped fever, and the majority of those attacked died; an instance of intermittent or of remittent fever was of extremely rare occurrence amongst the black troops, who were employed in common with our soldiers; of which most ample proof was afforded during the rebellion of the native population of the interior of that island, between 1817 and 1819.

[2] In Ceylon, during the period just mentioned, whilst our soldiers were recovering their health in the cool hill forts of the mountainous parts of the island, the negroes there were dying of pulmonary consumption.

ferred after careful examination to endeavour to detect the effects of diseased action. In no instance were there any indications of such action observed; the lining membrane contiguous was smooth, and there was no thickening where the ends of the band were inserted.

The basilar artery, in the manner in which it is formed, and the thinness of its coats, may be considered as approximating to a vein. The similarity is increased by the peculiarity in question. Bands of the same kind are not uncommon in the longitudinal sinus, and more delicate bands and fibres are frequently met with in the heart in the right auricle, especially about the fossa ovalis, and in connection with the eustachian valve.

Whether this peculiarity of structure has a decided use, I am not prepared to say. In each instance in which a band or fibre presents itself, support is afforded—additional strength is imparted. The band I have described as occasionally occurring in the basilar artery must necessarily have this effect.

It was in the month of June, 1837, that my attention was first attracted to the subject. Since that time, during a period of about sixteen months, I have taken every opportunity that has offered to examine the basilar artery. In ninety-eight *post mortem* examinations at which I have been present, made in the General Hospital, at Fort Pitt, in the time specified, I have met with it in seventeen instances. In nine, death was owing to pulmonary consumption; in two, to malignant tumour; in the remainder, in each instance, to a different disease. The ages of the individuals varied from 19 to 59; two were 25, two 35; in point of age there was no other accordance amongst them.

I shall give the results in a tabular form, and also the the proportional frequency of marked difference of size of the vertebral arteries to which my attention was at the same time directed.

TABLE I.

Period.	Number of bodies examined.	Instances of band in basilar artery.	Instances of left vertebral larger than right	Instances of right larger than left.
1837.				
June	5	3	4	1
July	16	6	9	1
August	13	2	—	1
September	9	1	2	3
October	5	1	1	—
November	8	1	—	1
December	5	—	2	—
1838.				
January	6	1	2	—
February	3	—	—	—
March	1	—	—	—
April	1	—	1	—
May	8	1	1	—
June	4	—	1	—
July	5	—	1	—
August	9	1	2	1
	98	17	26	8

In one instance of the total seventeen, a short filament proceeded from the intersecting band, its end floating loosely.

In two cases out of ninety-eight, there was another peculiarity, which I am not aware has ever been noticed, viz.—an opening or foramen between the two vertebral arteries, in the septum formed by their juxtaposition posterior to the basilar artery. In each instance it was sufficiently large to admit a surgeon's probe.

In three instances amongst the total number, each vertebral was as large as the basilar artery,—which was of the usual size of this artery; and the vertebrals did not appear to be diseased.

The proportion in which the left vertebral artery was found larger than the right is so great, viz. in the ratio of 26 to 8, that it can hardly be considered accidental. But on what the circumstance depends, I am entirely ignorant. At one time, I supposed that it might be connected with the difference of origin of the right and left subclavian; but the notion was not supported by facts. In two instances (the only ones observed) in which the left vertebral took its origin immediately from the aorta, between the carotid and subclavian, it was smaller than the right vertebral, taking its origin from the subclavian of that side,—and that subclavian, as usual, from the arteria innominata.

In the table I have given the results of my observations monthly, for a particular reason, namely, for the purpose of showing how much more frequently the peculiarities of structure referred to occurred in the bodies which died in one month than in another. The greater frequency of rare occurrences at one time than another is a circumstance extremely curious and mysterious, and I have often been impressed by it, as, I believe, others have been, both in hospital practice, and engaged in anatomical researches. I shall mention a few instances, from memory, as examples of such impressions.

1. An oblique opening in the fossa ovalis of the heart, occasionally of such frequent occurrence as to give the idea of its being normal.

2. The eustachian valve of large dimensions in the adult, nearly as large in proportion as in the fœtal heart.

3. A fibrous filamentous connection between the eustachian valve and the auricular septum.

4. The mouth of the coronary vein destitute of a valve.

5. Delicate tendinous filaments or threads, bordering the semilunar valves, especially of the aorta, giving the idea of an atrophied state.

6. The ligamentous cord, the remains of the ductus arteriosus, ossified.

7. Fibrinous concretions containing a purulent-like matter, formed during life, in the iliac and femoral veins, and also in the ventricles of the heart, most frequently in cases of phthisis.

8. Varicose lacteals, also in phthisis.

9. Softening of the brain, especially of the fornix, in this disease.

10. Ulceration of the larynx, also in this disease.

11. Pneumathorax from perforation of the pleura in connection with a tubercular excavation.

12. Peritonitis, from perforation of intestine, in connection with very limited local ulceration.

13. Ulceration and perforation of the appendicula vermiformis and consequent peritoneal inflammation.

14. Softening and wasting of the articular cartilages, especially of the patellæ.

The list might be very much extended. It may perhaps be said, that there is deception in this matter ; and that the asserted greater frequency of occurrence of any peculiarity of structure, or lesion, independent of obvious causes, is more apparent than real, and that were the same attention constantly given in search, the irregularity of their occurrence would cease. This may hold good in some instances ; but I cannot admit that it is applicable in those I have enumerated, and more especially in the peculiarity of structure of the band in the basilar artery, which led to the remark. In each case, the artery was carefully examined, and the result noted down at the time, and that specially. And, moreover, in relation to the subjects of observation, what could be less favourable for uniformity of result ? The individuals were not of any particular family or race ; they were men of different regiments, English, Irish, Scotch, brought to the general hospital, the invaliding station of the army, from different parts of the globe, in a manner approaching to the accidental as nearly as possible.

I am disposed to believe, that were the pathological anatomist engaged in extensive and minute research, to institute a new series of observations on organic changes, analogous to that which Sydenham conducted on ordinary maladies, he might arrive at the conclusion, that there are organic constitutions prevalent at times, not less than atmospheric, and which (however produced) may be as much concerned in the origin of chronic disease, as the atmospheric influences are in the acute.

I shall give in a tubular form some of the results of my experience bearing on this subject, drawn from my notes of the various post-mortem examinations which I have attended, during a period of nearly eighteen years, namely, from May, 1821, when I commenced the practice of making a note of every fatal case, in which there was an examination of the body after death. My experience has been chiefly confined to our military hospitals, indeed at home it has been entirely so restricted ; on foreign stations, it extended to the native population, especially in Malta, where the civil hospital offers an ample field for research.

TABLE II.

Stations.	Years.	Number of bodies examined.	Instances of tubercles in the lungs.	— of ulceration of larynx with do.	— of ulceration of larynx without do.	— of pneumathorax.	— of varicose lacteals.	— of aperture in fossa ovalis.
England, Fort Pitt,	1821	29	11	—	—	1	—	1
—	1822	61	33	19	—	—	6	10
—	1823	40	20	7	—	1	—	2
—	1824	10	5	2	1	—	—	--
Ionian Islands.	1824	18	2	—	1	—	—	2
—	1825	36	5	2	—	—	—	1
—	1826	9	3	—	—	—	—	—
—	1827	33	8	1	—	—	—	3
—	1828	6	—	—	—	—	—	--
Malta.	1828	32	5	1	—	—	1	5
—	1829	19	4	1	—	—	--	3
—	1830	41	5	1	1	—	—	9
—	1831	54	19	3	—	1	2	8
—	1832	55	8	1	—	3	--	7
—	1833	49	7	9	—	1	--	5
—	1834	57	15	5	—	3	1	5
—	1835	10	4	1	—	—	—	—
England, Fort Pitt.	1835	31	20	2	—	2	—	1
—	1836	72	49	18	—	1	—	8
—	1837	81	50	15	1	5	--	8
—	1838	43	45	11	—	1	—	3
Total,	18	786	318	99	4	19	10	82

On the results contained in this table, I shall restrict myself to a few remarks.

The very large proportional number of cases in which tubercles were found in the lungs, viz. 40 per cent. of the whole, may excite surprise. I must confess it had that effect on my mind, and the more so, as no doubtful instances were admitted: I rigorously rejected every example not coming under the denomination of the consumptive tubercle—that is, a tubercle, albuminous in composition, admitting of induration by boiling, as pointed out by Dr. Abercrombie, and of softening in the progress of disease, giving rise to vomicæ and excavations in the lungs. The melanotic tubercle was excluded, and also certain concretions more or less resembling tubercles, whether consisting principally of phosphate of lime and the other materials of bone, or of a nature approaching to cartilage. I may also remark, that no cases were admitted as supposed in-

stances of tubercles, from the presence merely of cavities in the lungs. Cavities existing unaccompanied by tubercles were inferred to be examples of pulmonary abscess, of which several instances occurred.

No doubt, the astounding frequency of tubercles recorded in the table, is partly owing to the description of cases sent to the general hospital, at Fort Pitt, where a considerable proportion of the whole mortality under observation occurred.   But, making the most ample allowance, on this account, I apprehend the conclusion is unavoidable, that the existence of tubercle is far more frequent than is commonly supposed, and the reported deaths from phthisis would indicate.

Of the four instances of ulceration of the larynx unaccompanied by tubercles, the first was complicated with empyema and purulent effusion into the pericardium to the extent of three pounds and a half; the second was associated with melanotic tubercles in the lungs ; and the third and fourth were connected with small-pox.

All the instances of pneumathorax, with the exception of one, occurred in cases of tubercular phthisis, and originated in a communication of a valvular kind being established between the pleura, and a bronchial tube by ulceration, commonly through the medium of a cavity.   In the one exception, a similar communication was detected, the consequence of a partial destruction of lung, from an abscess in the liver penetrating and bursting into the lung through the diaphragm.

Of the large number of examples of aperture in the fossa ovalis, all were oblique, with the exception of three.   Of the three direct, one was sufficiently large to admit the fore-finger, and two to admit the end of the little finger.   In neither instance was there the slightest appearance of the morbus cæruleus.   The subject of the first mentioned and most remarkable example was an old soldier, who for many years enjoyed excellent health.   In these cases, it appears inevitable that there must have been an admixture of venous and arterial blood in the auricles.   In the examples of the oblique passage, the probability is, that no blood flowed from one auricle into the other.   In one instance, in which the oblique aperture was sufficiently large to receive the end of the little finger, the right cavities of the heart were found distended with coagulated blood, and the left empty.

XXVII.

OBSERVATIONS ON THE FLUID IN THE VESICULÆ SEMINALES OF MAN.

I believe I am justified in stating, that there is still a difference of opinion amongst physiologists respecting the nature of the fluid

of the vesiculæ seminales, especially in this country, where the authority of John Hunter is necessarily high and influential.

The difference of opinion alluded to is, whether the fluid in question is secreted by the testes, or by the vesiculæ; and whether, in consequence, the vesiculæ are to be viewed chiefly as reservoirs or merely as glands.[1]

Conceiving that some light might be thrown on the subject by the examination of the fluid in the vesiculæ and in the vasa deferentia, after death, in a variety of cases, I have availed myself, for the purpose, of such opportunities as have offered in the general military hospital, at Fort Pitt, under my superintendence. And I shall now relate the results—with the belief, that they may aid in settling the disputed question; and, also with the hope, that they may be found to be not entirely devoid of interest in connection with an obscure branch of pathological inquiry.

It is necessary for this double purpose to prefix in each instance a slight notice of the fatal case. I shall make no selection of cases; but give them nearly in the order as to time in which they occurred. And, I may remark, that, as every fatal case was subjected to a post-mortem examination, according to the usage of the hospital, these brought forward (the total deaths from the 22d September, 1838, to the 6th of the ensuing December) afford a tolerably correct example of the general mortality of this hospital—the patients in which are chiefly invalids labouring under chronic diseases, incapacitating them for farther military service.

1.—Aged 30; previously labouring under pulmonary disease and an impaired constitution, was admitted with pneumonia, and died on the third day. On dissection, thirty-nine hours after death, the inferior lobe of the right lung was found hepatized, weighing nearly three pounds, the superior and middle only half a pound. There was a tubercular excavation and numerous tubercles in the left lung—and small cavities and a pretty distinct cicatrix of a vomica in the upper part of the superior lobe of the right lung. The body was not emaciated.

The vasa deferentia and vesiculæ seminales were removed six hours after death. About a drop of fluid was obtained from each vas deferens, which accorded in appearance with the received

[1] Vide Dr. Bostock's Elements of Physiology, Vol. iii. p. 7, and foot-note, where the question is left unsettled by the learned author. He thus expresses himself on the subject:—"Besides the testes, the vesiculæ seminales, both from their size and their situation, have been supposed to perform some important part in the function of generation, although it has been difficult to ascertain the exact nature of the purpose which they serve. The opinion formerly entertained was, that they are merely reservoirs, in which the semen is deposited as it is secreted. In consequence, however, of the observations of Hunter, who remarked that the fluid contained in these cavities appeared to be different from that found in the testes, many of the later anatomists have supposed that the vesiculæ seminales produced a secretion of a peculiar nature, the use of which may probably be to dilute the semen, or to add to its bulk."

description of the spermatic fluid. Examined with a microscope, constructed by that excellent maker Mr. Ross, and using an object glass of one-eighth inch focal distance, it was found to contain numerous animalcules, some of them in active motion. The vesiculæ contained a considerable quantity of fluid. Some of it was collected after six hours; another portion later, after forty-two hours. The former did not appear to me to differ from the fluid of the vasa deferentia. It abounded in spermatic animalcules, some of which were alive and active: on standing a few hours it separated into two parts, one opaque that had subsided, the other transparent; and this was copiously precipitated by alcohol, and rendered of a consistence almost gelatinous. The last collected had a brownish tinge; it, too, abounded in animalcules; but they were motionless and dead; warmth had no effect in reanimating them.

2.—Aged 57; a violent maniac, who died thirteen days after admission; the disease was of about a fortnight's duration—a third attack. The dissection was made fifty-seven hours after death. The body was much emaciated. The lateral ventricles of the brain were greatly distended with fluid; they communicated freely —the septum lucidum having been destroyed by disease. No other well-marked lesion was discovered.

A very minute portion of fluid was obtained from the vasa deferentia. It was of the colour, and very much of the appearance and consistence of pus. It contained very many spermatic animalcules—all dead. The fluid from the vesiculæ was small in quantity, browner than that from the duct, as if slightly tinged by the colouring matter of the blood. It contained vestiges in abundance of spermatic animalcules, and a few of distinct form.

3.—Aged 39; died of pulmonary consumption, complicated with pneumathorax of the right side and empyema.[1] The dissection was made thirty-six hours after death. The body was much emaciated. Besides the disease in the lungs, the ileum in its inferior part was severely ulcerated, and the cæcum slightly. There had been diarrhœa in the last stage of the disease.

The vasa deferentia, and the vesiculæ seminales, were removed and examined six hours after death. The contents of the latter were small in quantity, and of nearly gelatinous consistence. The fluid of the ducts was more liquid, and nearly of its usual cream or purulent-like appearance. In neither of them could any animalcules be discovered, or even vestiges of them, or any distinct globules.

4.—Aged 20; died of pulmonary consumption. Extreme debility and emaciation marked the last stage of the disease, in which diarrhœa and night-sweats alternated. The dissection was made twelve hours after death. No notable lesion was detected inde-

[1] In this case, as in every other of pneumathorax which I have yet examined, the air in the pleura was derived from the atmosphere through a valvular ulcerated opening in the lung.

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pendent of the pulmonary disease, which was vast and various—a
complication of tubercles and cavities, of œdema and hepatization.
The intestines were not ulcerated.

The vesiculæ and vasa deferentia were examined eleven hours
after death.   The former contained a small quantity of fluid, which
was brownish, thick, and slightly viscid—the latter an extremely
minute portion, like thin starch.   In both, under the microscope,
there appeared, as it were, the fragments of animalcules and nu-
merous globules.

5.—Aged 32 ; died of latent pulmonary consumption, after hav-
ing been a patient in the lunatic asylum of the establishment about
a month.   The dissection was made fourteen hours after death.
The body was much emaciated.   Besides tubercles in different
stages, and excavations in the lungs, and partial hepatization, there
was little other organic disease.   Diarrhœa had not preceded death ;
there were, however, some small ulcers in the colon, and marks of
old ulcers healed.

The vesiculæ and the vasa deferentia were examined sixteen
hours after death.   A small drop of fluid was obtained from the
latter, of cream or purulent-like appearance, which abounded in
seminal animalcules.   The vesiculæ were moderately turgid ; the
fluid in them was also opaque, white, like purulent matter, and
abounded in animalcules.   The animalcules in each instance were
dead.   Neither fluid changed the colour of litmus or of turmeric
paper.

6.—Aged 39 ; died of gangrene of the superior lobe of right
lung, complicated with effusion into the pleura, and with ascites.
There were sixty-one ounces of turbid serum in the right pleura,
and seven pints of serum in the cavity of the abdomen.   The liver,
spleen, and pancreas, were denser than natural ; the weight of the
liver was only two and a half pounds.   There was a large ulcer
in the cæcum, and numerous marks of old ulcerations healed in
the colon and rectum.   Chronic dysentery preceded the fatal dis-
ease.   The dissection was made twenty-eight hours after death.

The right vas deferens was examined two hours after death,
and the vesiculæ half an hour later.   A small drop of fluid was
obtained from the seminal duct.   It resembled in appearance the
purest purulent matter : it had no distinct smell ; it rendered tur-
meric paper slightly brown.   Under the microscope it appeared to
consist chiefly of minute globules, very much smaller than those of
purulent matter, and to be entirely destitute of animalcules.   The
vesiculæ were so shrunk and collapsed, that difficulty was experi-
enced in collecting sufficient fluid even for microscopic examina-
tion.   No animalcules could be detected in it, and no distinct glo-
bules.   It was colourless and not viscid—more like serum than
mucus ; and it was alkaline, and changed distinctly the colour of
turmeric paper.

7.—Aged 42 ; of twenty years' military service ; died of pulmo-
nary consumption.   The dissection was made thirty-seven hours

after death. The body was much emaciated. The lungs weighed five pounds. Besides tubercular excavations and a vast quantity of tubercles in different stages of progress, there was partial œdema of the lungs and hepatization. There were large ulcers in the jejunum, ileum, and colon; diarrhœa did not precede death. The prostate gland contained a sinus, which opened into the neck of the bladder. The testes were adhering to the tunicæ vaginales; and the substance of the right testis was partially indurated, as if from the effect of common inflammation.

The vesiculæ seminales were moderately distended with fluid, of a light brownish hue, slightly turbid and opaque. It abounded in well-formed spermatic animalcules, and contained a few blood corpuscles, or particles extremely like them. It had no effect either on turmeric or litmus paper. An extremely small portion of fluid was obtained from one of the vasa deferentia, which, in appearance, was very like that of the vesiculæ. Under the microscope, a few animalcules were seen in it, many blood corpuscles and some smaller particles.

8.—Aged 32; died of bronchitis, complicated with hepatization of the right lung, and an aneurism of the arch of the aorta pressing on the trachea. The dissection was made thirty-two hours after death. The body was not emaciated. The bronchia were nearly filled with muco-purulent matter. The right lung weighed three pounds; the greater part of it was hepatized in different degrees, and it contained a few clusters of granular tubercles. No well marked lesion could be discovered in the abdominal viscera.

The vesiculæ were moderately distended with fluid of purulent-like appearance, which abounded in seminal animalcules. The fluid obtained from one of the vasa deferentia, extremely minute in quantity, was found to contain a few animalcules, and very many particles smaller than those of the blood.

9.—Aged 33; died of pulmonary consumption. The dissection was made fifteen hours after death. The body was sub-emaciated. Besides numerous tubercles in different stages of progress, and several large cavities in the lungs, there was partial œdema and hepatization of them. The liver weighed six and a half pounds, and contained some fatty matter. There were no dropsical symptoms. There were numerous ulcers in the ileum and colon. There had been no diarrhœa.

A small quantity of fluid was found in the vesiculæ, of a brownish hue and semi-opaque. It did not change the colour of turmeric or of litmus paper. It abounded in spermatic animalcules. The very little fluid procured from a vas deferens was like dilute purulent matter in appearance; it contained a few animalcules, and very many minute globular particles.

10.—Aged 20; died of pulmonary consumption. The dissection was made twenty-six hours after death. The body was much emaciated. Both lungs abounded in tubercles, had a large excavation in each superior lobe, and contained many small vomicæ, and

the right lung in addition was extensively hepatized. There was extensive superficial ulceration of the larynx and trachea. The ileum and colon were deeply as well as extensively ulcerated. The appendicula vermiformis was perforated by ulceration, and through the medium of a sinus communicated with the colon by an ulcerated opening. There were thirteen ounces of a mixture of purulent matter and serum in the cavity of the pelvis. Severe diarrhœa and tormina preceded death.

The vesiculæ and vasa deferentia were examined four hours after death. The former afforded only a small quantity of fluid; what was first pressed out was like thin starch in appearance; what was last expressed was gelatinous. The former, under the microscope, was found to contain many animalcules, mixed with globular particles and small masses of mucus. The latter contained very few animalcules, and consisted chiefly of mucus. The fluid from the vas deferens was very like that which flowed first from the vesiculæ, excepting that it was without any tint of brown which that fluid possessed. Under the microscope, only one animalcule, perfect in form, could be detected; fragments of others, as it were, appeared, and some globules, and a good deal of transparent mucus in minute masses.[1] The fluid of the vesiculæ and of the vas deferens was distinctly alkaline; each changed the colour of turmeric paper.

11.—Aged 27; died of pulmonary consumption. The dissection was made ten hours after death. The body was much emaciated. The lungs were enormously diseased; besides containing large cavities and many vomicæ, and a vast quantity of tubercles in different stages of progress, they were partially œdematous and hepatized. There were a few ulcers in the ileum, and a thickened granular and partially ulcerated state of the rectum. There had been diarrhœa in the last stage of the disease.

Very little fluid was found in the vesiculæ seminales. It was partly thin, of a light brownish hue; and partly thick and gelatinous,—the latter from the fundus, the former from the anterior portion. Under the microscope, a few spermatic animalcules were detected in the thin fluid, mixed with what appeared to be fragments of them. No animalcules were observed in the gelatinous fluid; it seemed to consist principally of mucus. Neither fluid had any effect on turmeric paper. From the vas deferens hardly sufficient fluid could be procured, even for microscopic examination. It contained a few minute globular particles without any animalcules.

12.—Aged 33; died of diffuse cellular inflammation of the neck, with suppuration of the cervical glands. The dissection was made twenty-six hours after death. A superficial cavity was found in the cerebrum, under the posterior part of the right parietal bone, which

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[1] In this and every other instance the vas deferens was removed for examination, before entering the cavity of the abdomen; the cord was laid bare, the duct separated, and a ligature applied previous to dividing it.

contained about a drachm and a half of fluid, which became turbid
on the addition of nitric acid. There had been no suspicion of
cerebral disease during life. The cellular tissue of the neck gene-
rally was saturated with thick purulent matter; and most of the
cervical glands were diseased and contained abscesses. The right
sterno-cleido-mastoideus muscle, on its under side, appeared wasted
and corroded, and presented a suppurating surface. A small mass
of fibrin, softened internally, adhered to the inner coat of the right
internal jugular vein. Neither the integuments, larynx, œsophagus,
or pleuræ, participated in the disease. During life it had not been
suspected. The left femoral and the right iliac and femoral veins,
were obstructed with coagula, which in the inferior parts consisted
of crassamentum, and in the superior of fibrin. In the upper part of
each vein the fibrinous concretion contained a cavity in which was
a semifluid, having a good deal the character of purulent matter,
and which would commonly be called purulent matter.[1] The lower
extremities were œdematous, the right more than the left,—but even
the right extremity only in a moderate degree. There was little
hair on the pubis, or on the chin; the partes naturales were all small;
the larynx was small; the skin delicate. According to his comrades,
he had always shown an aversion to the sex.

A very minute portion of fluid only could be procured from the
vasa deferentia, which, under the microscope, exhibited numerous
small particles, and a few larger globules,—but no spermatic animal-
cules. The fluid of the vesiculæ was also small in quantity and
destitute of animalcules. It was of a light brownish hue, slightly
opaque, contained some globules, and did not change the colour of
turmeric or of litmus paper. The fluid from their fundus was most
gelatinous, and appeared to consist chiefly of mucus.[2]

13.—Aged 29; died of pulmonary consumption, complicated with
peritoneal inflammation. The dissection was made twenty-seven
hours after death. The body was exceedingly emaciated; the lungs
were very voluminous and heavy; they contained a vast number of
tubercles in different stages, some small cavities, and one large one.
This cavity was lined with a false membrane, which was even
spread over the abrupt mouths of two or three bronchial tubes, and
it was empty. Whether it was distended with air, or its sides
pressed together, was not ascertained. Eight pints of serum were

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[1] It sustained this character subjected to the best tests: Thus, agitated
with water, a white matter subsided, very little prone to putrefaction, irides-
cent when held before a light, globular under the microscope. The differ-
ences were rather in degree than kind, indicating, as it were, if the expres-
sion may be used, an imperfect purulent matter; thus, it putrefied more readily
than purulent matter; was only just perceptibly iridescent, and its particles
were more irregularly globular and less compact. The mode of formation of
this kind of fluid, with which pathologists are now tolerably familiar, is a
curious problem, and deeply interesting in many of its relations.

[2] The vesiculæ seminales, and their contents, in this instance, resembled
those of such castrated animals as I have hitherto examined.

collected in the cavity of the abdomen. There were very numerous
adhesions between the viscera. The omentum abounded in granu-
lar tubercles, and resembled an elongated pancreas. The cæcum
was misplaced, and with the colon was confined to the left side.
The ileum, appendicula vermiformis, and colon, were severely
ulcerated. The appearances indicated a perforation of the appendi-
cula by ulceration, afterwards closed by adhesion, and which pro-
bably gave rise to the peritoneal inflammation. The abdominal
disease was not suspected during life.

No fluid could be obtained from the vasa deferentia ; one was ex-
amined three hours after death. The testes were of natural size,
and not apparently diseased. The vesiculæ contained very little
fluid. It was thick, gelatinous, nearly transparent and colourless,
and did not change the colour of turmeric or litmus paper. Under
the microscope some globules were seen in it, but no animalcules.

14.—Aged 27 ; died of pulmonary consumption. The dissection
was made thirty-two hours after death. The body was much ema-
ciated. The lungs were enormously diseased; besides a large ex-
cavation in each superior lobe, and several smaller ones, and a vast
quantity of tubercles in progressive change, there was partial œdema
and hepatization, and thickening of the bronchia. One arytenoid
cartilage was laid bare by ulceration ; the left nervus vagus was
partially atrophied under the pressure of an enlarged bronchial
gland. There were numerous large granulating ulcers in the
cæcum and ascending colon; the vermiform appendix was severely
ulcerated, and the lower part of the ileum in a less degree. A short
time before death, the bowels were regular ; previously there had
been diarrhœa.

The fluid obtained from the vasa deferentia, less than a drop in
quantity, had a healthy appearance ; it contained a few spermatic
animalcules, and many globules, most of them very small, a few
large. The vesiculæ were moderately distended with a fluid of a
just perceptible brownish hue, of the consistence of mucilage, and
slightly viscid. It did not change the colour of turmeric paper. A
small number of spermatic animalcules were detected in it, and
many large globules, which did not disappear on dilution with
water.

15.—Aged 27 ; died of the effects of inflammation of the pleuræ
and lungs. The dissection was made thirty-six hours after death.
The body was sub-emaciated. There were twelve ounces of serum
in the left pleura, and sixteen ounces in the right, with a sediment
in each of flakes of lymph. Both lungs were partially adhering,
and much heavier than natural ; the left was œdematous to a con-
siderable extent, and the right hepatized. Neither contained tuber-
cles. In the superior lobe of the right lung, there was a minute
cavity, surrounded by an indurated, dark, puckered structure, con-
veying the idea of a cicatrix of an abscess. The cæcum and as-
cending colon were red and rather flabby. The spleen, pancreas,
and kidneys, were obscurely diseased. During life there had been

no supicions of thoracic disease. Symptoms of disease of various kinds had existed at least nine months before death ; anasarca pre-vailed at one time, and ascites ; latterly the bowels were irregular, occasionally relaxed and passing blood, occasionally constipated.

The vasa deferentia were examined twelve hours after death. The very minute portion of fluid obtained from them was destitute of distinctly formed animalcules ; it contained globules of different sizes, some of them very like fragments of the peculiar entozoa,—the circular portion without the filament. The vesiculæ were ex-amined thirty-six hours after death. They contained pretty much fluid, of a just perceptible brownish hue, and of a gelatinous con-sistence. Very many well-formed animalcules appeared in it. It did not change the colour of turmeric paper.

16.—Aged 33 ; died of complicated organic disease of the bones, lungs, pleura, heart, peritoneum, &c. The dissection was made thirty-six hours after death. The body was exceedingly emaciated. The os frontis was carious, and in two places penetrated through by ulceration ; lymph was deposited underneath on the dura mater. The left clavicle was enlarged, softened, spongy, and unusually vas-cular. At the base of each semilunar valve of the aorta was a small bony concretion. The heart appeared atrophied. The pleuræ were studded with small, white, firm tubercles. The right pleura contained sixteen ounces of serum ; the left four ounces. In the inferior lobe of right lung, there were two circumscribed masses of coagulated blood. The cavity of the abdomen contained twelve pints of serum. The abdominal peritoneal lining generally was studded with tubercles similar in character to those of the pleuræ, as was also the omentum, which, gathered into a long mass retain-ed by adhesion, resembled a pancreas in its appearance. The kid-neys were small and obscurely diseased. The left iliac and femoral vein, and the right femoral, were completely obstructed by coagula, in part consisting of fibrin, softened, and of imperfect purulent-like appearance, and in part of crassamentum little altered. There were no tubercles in the lungs ; no ulcers in the air-passage or ali-mentary canal.

The vasa deferentia were examined six hours after death, and the vesiculæ thirty-six hours after. The very little fluid procured from the former, contained numerous minute globules, and some of a larger size, but no animalcules. No animalcules either were found in the fluid of the vesiculæ, merely globules of different sizes. The fluid contained in them was in a very small quantity, of a light brownish hue, and it did not change the colour of turmeric paper.

17.—Aged 31 ; died of pulmonary consumption. The dissection was made twenty-five hours after death. The body was greatly emaciated. Besides tubercles, which abounded in the lungs, and tubercular excavations, there was some œdema of these organs, and hepatization and emphysema,—the last to a considerable extent.'

¹ Contiguous to the part where the emphysema was most remarkable, was

with much redness of and purulent discharge on the bronchia. The intestines were not ulcerated.

The vasa deferentia and vesiculæ were examined twenty-seven hours after death. The vesiculæ were considerably distended with fluid, which was of a brownish tint, and did not change the colour of turmeric paper. It differed in consistence, according to the part from whence taken; that from the fundus was of the consistence of thick mucilage; that from the anterior part was more fluid; each abounded in well-formed animalcules, and in globules of a large size. About a quarter of a drop only of fluid could be procured from each vas deferens. No animalcules could be discovered in it, but globules of different sizes, and some resembling fragments of animalcules.

18.—Aged 49; died after an illness of three days, in consequence of inflammation of the brain; had been previously in good health. The dissection was made fourteen hours after death. The body was stout and muscular. A large quantity of turbid serum with a sediment of purulent matter was found in the ventricles, and a considerable deposit of lymph with which purulent matter was mixed, on the base of the brain, the medulla oblongata, and on the upper part of the spinal cord. The abdominal and thoracic viscera generally were sound.

The testes and vasa deferentia were examined ten hours after death; the vesiculæ seventeen hours after. The fluid obtained from the vasa deferentia abounded in animalcules, some of which were alive and in languid motion. The fluid of the vesiculæ also abounded in them; they were all dead. This fluid had a brownish tint, and reddened litmus.[1]

19.—Aged 30; died of pulmonary consumption. The dissection was made twenty hours after death. The body was much emaciated. In both lungs granular tubercles abounded, and there was a large cavity in the superior lobe of each. The liver, spleen, kidneys, and supra-renal glands, were unusually firm. The liver weighed seven pounds, and contained some fatty matter. The spleen was about twice its natural size, and a portion of it dried on paper left a faint grease-stain. The ileum, cæcum, appendicula, vermiformis, colon, and rectum, were partially ulcerated; and some of the ulcers in the large intestines had penetrated through all the coats, excepting the peritoneal, and, indeed, this coat was penetrated through in the appendicula, where it happened to adhere in the right iliac fossa.

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a small cavity, which communicated by an ulcerated opening with the air-cells of the lungs. When the part was punctured under water, several cubic inches of air escaped from the emphysematous portion. It consisted of eight of azote by measure, and two of carbonic acid gas. In many other instances in which I have examined the air of emphysema of the lungs, I have found it similar.

[1] In this case, the prostate contained a good deal of fluid, corresponding to Meckel's description of it in its healthy state. It was of a fawn colour, opaque, slightly viscid; under the microscope it was found to abound in pear-shaped particles, nearly equal in magnitude to blood-corpuscles.

There had been no dropsical symptoms; no peritoneal inflammation; diarrhœa had been troublesome in the last stage of the disease.

The vasa deferentia and vesicula were examined twenty-two hours after death. One vesicula contained pretty much fluid, the other very little. The fluid was of a just perceptible brownish hue, and of the consistence of mucilage; it contained a large number of animalcules and some globules, and did not change the colour of turmeric or litmus paper. An extremely minute quantity of fluid was procured from the vasa deferentia, of cream-like appearance; it contained some globules, and many minute particles, but, no animalcules.

20.—Aged 41; died in consequence of the rupture of an aneurism of the thoracic aorta, not suspected during life. The dissection was made thirty-three hours after death. The body was muscular and robust as in perfect health. In the left pleura, into which the aneurism had opened, there were three and a half pints of serum, and five and a half pints of soft crassamentum. The viscera generally were sound.

The vesiculæ were examined thirty-eight hours after death, and the right vas deferens fifty-eight hours after. The fluid in the former was pretty abundant, of a grayish hue, without tint of brown, and of the consistence of common mucilage. It contained many animalcules; did not change the colour of turmeric or of litmus paper; was coagulated by alcohol, and rendered a little more opaque by momentary boiling. In the drop of opaque fluid obtained from the vas deferens, no animalcules could be detected; it contained very many minute particles, and many particles of an irregular form.

In all these cases the testes were examined, as well as the vesiculæ and the vasa deferentia. Excepting in two instances, viz. the 18th and 20th, no animalcules could be seen in the fluid expressed from the divided substance of the gland. The fluid, when it could be obtained in sufficient quantity for accurate observation, was transparent, generally contained globules nearly equal in diameter to the blood corpuscles, and invariably contained dense particles, apparently spherical, very much smaller, from twelve to fifteen times smaller, and which, it may be conjectured, were the ova of the spermatic entozoa.[1] In the two instances in which spermatic animalcules were found in the fluid of the tubuli, the quantity of the fluid was greater than in the others.[2]

[1] I am disposed to consider this as probable, partly in consequence of the peculiarity of appearance of the particles, differing from any particles which I have hitherto seen in other parts of the body,—and partly from analogy. MM. Prevost and Dumas, from their very extended and ingenious researches, have adopted a different conclusion, viz. that the spermatic entozoa are the results of secretion, and that the testes are the secreting organs;—a doctrine, perhaps, equally difficult to prove or disprove. The circumstance of the animalcules having been detected in the majority of the preceding cases in the vasa deferentia and vesiculæ, when they could not be found in the testes, appears to me more in favour of their origin *ex ova* than by secretion.

[2] Probably in health, these animalcules exist invariably in the testes of

What are the inferences to be drawn from the preceding observations in relation to the question started concerning the nature of the fluid, and the use of the vesiculæ seminales?

The first inference that appears to me unavoidable is, that the vesiculæ are seminal reservoirs, according to the old opinion on the subject and that which is still most commonly entertained by the continental physiologists. And next, that they are not merely reservoirs, but are also secreting organs, furnishing mucus, and perhaps some other fluid, for admixture with the semen.

The first inference is supported by the general resemblance, in several cases, of the fluid of the vasa deferentia and of the vesiculæ, and of the existence of the characteristic spermatic animalcules in the fluid of the vesiculæ, in every instance in which they were detected in the fluid of the vasa deferentia.[1] Hunter does not mention having used the microscope in his inquiry. If he had, he could hardly have failed to have arrived at a different conclusion.

The second inference is supported by there being a certain difference in almost every case between the fluid of the vesiculæ and that of the vasa deferentia, and especially by the circumstance, that the difference of quality is most perceptible in the fluid of the fundus,—where most out of the way of being readily mixed with the fluid of the testes. What the exact difference of qualities is between the fluid of the vesiculæ and of the vasa deferentia, and, it may be added, of the vasa deferentia and of the testes, in perfect health, remains to be ascertained. It can be determined only by careful examination and comparison in the instances of criminals who have been executed, or of persons who have been killed by accident, not labouring under chronic disease, and in the vigour of life. I am disposed to think that the difference will not be found very considerable, and that between the fluid of the vesiculæ and of the vasa deferentia, it will consist chiefly in the former being more dilute and perhaps more bland and mucous.[2] Hunter was of opinion that the fluid of the vesiculæ is naturally of a brownish hue. As an invariable quality this appears questionable. The sooner after death the fluid is examined, the less brown it is. I have given amongst

man contained in the tubuli. The results of the experiments of MM. Prevost and Dumas on twenty-five different species of vertebrate animals, may be adduced in support of this conclusion: (Vide Ann. des Sciences Naturelles, vols. i. and ii.) and I may add, that all the comparative trials I have yet made are in favour of it.

[1] I may add, that I have observed spermatic animalcules in the vesiculæ of the ram and bull, precisely similar to those found in their testes and vasa deferentia; and if I recollect rightly, they have been detected in the vesiculæ of some other animals by MM. Prevost and Dumas. Whether the vesiculæ of certain animals, however, have not a specific use, distinct from that of being merely reservoirs, appears to be deserving of further and special inquiry.

[2] That the vesiculæ are glandular, or possess mucous follicles, seems unquestionable; I have seen them with the microscope, and the mucous secretion is made manifest in disease.

the preceding cases, instances in which it was colourless, and those examples in which there was reason to consider it least altered in consequence of disease : and Mr. Hunter's best observations are in accordance, viz. two instances, the only ones given in which exami- nation was made very soon after death. He says, in a man killed by a cannon-ball, " the fluid in the vesiculæ was of a lighter colour than is usually found in men who have been dead a considerable time ; but it was by no means like the semen either in colour or smell." And, he adds, " in another man who died instantaneously in consequence of falling a considerable height, and whose body I inspected soon after the accident, I found the contents of the vesi- culæ of a lightish whey-colour, having nothing of the smell of semen, and in so fluid a state as to run out on cutting into them."[1] The colour which the fluid of the vesiculæ commonly exhibits in late post-mortem examinations is probably partly derived from the infiltration of some of the colouring matter of the blood after death, and, in part, perhaps occasionally, from a vitiated secretion of the follicles, or secreting surface of the vesiculæ, and of the adjoining vasa deferentia.[2]

It would be a work of supererogation to enter into a detailed ex- amination of the arguments brought forward by Hunter in support of the views he advocated relative to the vesiculæ and their con- tents : it has been ably done by Meckel in his Manual of Anatomy, by whom each argument has been seriatim answered, and in most instances, as it appears to me, in a satisfactory manner, but without reference to microscopical research.[3]

Admitting the fact, that the vesiculæ are, like the gall-bladder and bladder of urine, recipients, it may be viewed as a fortunate circumstance in our economy, and admirably adapted to the con- dition of *man*. Like the bile or the urine, so the spermatic fluid in the healthy adult appears to be in constant process of secretion, and to pass as it is formed into its appropriate reservoir, from whence, without disturbance of the system, in a state of continence, it is either pressed out and voided during the act of alvine evacuation, or it may be in part absorbed. Mr. Hunter, in accordance with the opinion which he had formed of the use of the vesiculæ, did not admit this. He believed that the fluid rather accumulated in the testes, and gave rise there to annoyance, requiring its evacuation by a disturbing act, a doctrine of some danger, especially to youth, in

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[1] Observations on certain parts of the animal economy. By John Hunter, 4to., London, 1786, p. 28.

[2] Occasionally after death, not only is the fluid of the vesiculæ found of a brownish discoloration, but also the inner surface of the vesiculæ, and of that portion of the vasa deferentia which is contiguous, and of similar structure ; indeed, I have seen the portion of the vasa deferentia just mentioned more discoloured than the vesiculæ themselves.

[3] Manuel d'Anatomie Générale, Descriptive et Pathologique. Par J. F. Meckel, traduit par A. Jourdan et G. Breschet. Paris, 1825. Tom. iii. p. 643, et seq.

its consequences. In confirmation of what is stated above, and in opposition to the docrine of Hunter, I may farther state, that I have frequently examined microscopically the fluid from the urethra following the alvine evacuation, and I have always found it, from a healthy person, abounding in animalcules, the majority of which have always been dead;[1] and thus, perhaps, seeming to indicate that the vesiculæ are cloacæ as well as reservoirs, and are essentially designed for man, to enable him to control and to exercise that moral check on the passions, by which he should be distinguished from brute animals, and without which no considerable advance can be made in civilization, or in elevation of individual condition and character.

Relative to the effects of disease on the fluid of the vesiculæ seminales, and on the spermatic fluid generally, the instances brought forward are too few to admit of extensive induction. They seem to show: 1st, that tubercular consumption, even in its advanced stage, has little effect in preventing the secretion of the testes, or the production of those animalcules, on which, there is much reason to infer, the active power of the semen depends; 2dly, that other wasting diseases, terminating in death, have the effect of arresting the secretion; 3dly, that the contents of the vesiculæ and vasa deferentia, under the influence of disease, retain longer their characteristic qualities than the contents of the tubuli; and, 4thly, that there is least fluid in the vesiculæ and in the vasa deferentia, and that it is most altered in instances of chronic diseases of the abdominal viscera, and especially of the intestines.

The subject of inquiry is in many respects curious, and some other of its relations are not uninteresting or unimportant. I shall allude to one only before concluding, and that connected with medical jurisprudence.

Admitting that spermatic animalcules are characteristic of and essential to healthy spermatic fluid, in certain doubtful criminal cases, probably, decisive evidence may be obtained by means of microscopical examination. The spermatic fluid undergoes change rapidly when exposed to the air, and even soon becomes putrid; but the spermatic animalcules, I find, resist change in a remarkable manner. In one instance, distinct remains of these animalcules were observed in putrid fluid, which had been kept ten weeks, at a temperature varying between 50° and 60° of Fahrenheit, and in another which had been kept a year and a half. In another instance, some fluid of the vesiculæ was applied to linen, and wrapped in paper and put by in a close drawer. It was examined the following day, at the end of a week, and after eighteen days, and each time animalcules were discovered under the microscope. The mode of making the trial was by saturating a small portion of the

[1] On rest, contrary to what Hunter states, I have found this fluid separate into two parts—a sediment containing the animalcules and opaque, and a clear supernatant fluid destitute of animalcules—thus analogous also to the spermatic fluid.

smeared linen with a few drops of water, and gently pressing out a drop for the experiment. Fragments of the animalcules were very distinct, and sufficiently characteristic; and on careful inspection, an entire animalcule here and there was observed. The application of these facts to the purposes of evidence does not require any comment.

In conclusion, it may not be amiss to allude to the views of Sir Everard Home on the subject of the fluid under consideration. They occur, detailed at great length and in a very circumstantial manner, in the fifth volume of his Lectures on Comparative Anatomy, and are well worthy of being referred to as a scientific curiosity. The opinion which Sir Everard Home there maintains is, that spermatic animalcules have no real existence; and that all inquirers who have asserted their existence, from the time of their discovery by Ham, in about 1677,[1] to the present period, have been in egregious error, having mistaken, as he believes, filaments of mucus for entozoa, and fallen thereby into wild theories. Sir Everard Home relates particularly the grounds on which he came to this conclusion, and the very great pains he took to endeavour to arrive at the truth;—how he made a journey with his able assistant and accomplished artist, Mr. Bauer, to Paris, expressly for the purpose of examining the spermatic fluid with a microscope of Chevalier, by which the peculiar entozoa had been seen by certain French savans;—how he procured an excellent instrument of the same kind, and employed it at home; and how, aided by Mr. Bauer, after examining the spermatic fluid of the fallow-deer, when in heat, during two seasons, under most favourable circumstances, he was unable to discover any animalcules in it, and was confirmed in his opinion of their non-existence.

At present, this opinion of Sir Everard Home's being unquestionably erroneous, it is not necessary either to criticise or controvert it. The reflection, however, which it gives rise to, (if I may judge from my own feeling,) is of a painful kind; especially in relation to his other microscopical observations—so beautifully represented by Mr. Bauer, and admirably engraved—in the accuracy of which now it seems a matter almost of impossibility to have any confidence. And this I think it right to point out particularly, considering the manner in which Sir Everard Home prefaced the most remarkable of the microscopical observations,—observations which, if correct, would, as he anticipated, have been of the greatest importance, and which could not fail producing a great change in our physiological doctrines. Take, as an example, his introductory remarks to his Croonian Lecture, for 1817, "On the Changes the Blood undergoes in the act of Coagulation," in which he en-

[1] Vide Leuwenhoeck's letter to Lord Brouncker, dated Nov. 1677, in the 12th volume of the Philos. Trans., in which he mentions that the discovery of spermatic animalcules in man was first made by Ham, and how he confirmed it, and discovered them also somewhat different in form in the semen of the dog, cat, and rabbit.

deavours to prove that the blood-channels, giving rise to vessels, are formed in lymph by carbonic acid gas, in the act of passing as it is disengaged, as he supposes, from blood in coagulating.

"It is not a little remarkable," (he says) "that in the first lecture of this kind, which I laid before the society in the year 1790, I should have endeavoured to show that a muscular fibre was too minute an object to be seen by the human eye, even assisted by the best magnifying glasses then in use; and that, in this lecture, I shall be able, by means of the great improvements that have been made in the use of the microscope, to show that a fibre, not larger in diameter than one of the globules of the blood, can be demonstrated.

"To the members of this society," (he continues) "who have so lately seen Mr. Bauer's drawings of the glandular apparatus peculiar to the Java swallow, of the internal membrane of the human stomach, exposing structures that were not known to exist, also of so small an object as the human ovum, in which is seen the seat of two of the most important organs of the body, (drawings rendered beautiful by their simplicity and distinctness,) it will readily suggest itself, that Mr. Bauer is the person to whom I consider we are indebted for those improvements. His whole life, I may say, has been employed in investigations of a similar nature; in plants, observing first the natural appearances, and then magnifying them in different degrees, and comparing, with the nicest discrimination, what was exhibited by one magnifying power with what was shown by that immediately above it,—and, where they did not correspond, employing the whole energies of his mind, with a patient labour almost beyond what is natural, in ascertaining the cause of the deception which must in one of them have taken place. To the observations of such a man upon subjects of this nature, if we are not confidently to place a reliance, how are we to give credit to the remarks that are made by common observers?

"I have said thus much," (he adds) "as an introduction to the observations that I am going to bring forward, for the public to know, whatever opinion they may form of them, they have been the results of long and unwearied research; and have been so frequently repeated as to satisfy Mr. Bauer of their correctness."[1]

The reader acquainted with the history of the microscope, and the powers of the best instruments which have been in use from the days of Leuwenhoeck to the present time, will smile at Sir Everard Home's mistaken views of the improvement of the instrument as alluded to by him, and at the test which he gives of the power of the microscope employed by Mr. Bauer;—the defectiveness of whose instrument, it must be inferred, was one of the chief sources of error to which those two very zealous inquirers were exposed. I fear it must be admitted of Sir Everard Home, that he was too ambitious of being ranked as a great discoverer, and that he had

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[1] Philos. Trans. for 1818, p. 172.

not the power of resisting the temptation of representing as a discovery what he believed himself, although not clearly demonstrated. His account of the human ovum, referred to in the preceding quotation, may be noticed as an instance of this his disposition. It is now believed by those who had the best means of correct information on the subject, that the supposed ovum—to which he attached so much importance, and of which there are such beautiful representations engraved in the Philosophical Transactions for 1817, from Mr. Bauer's drawings—was only the egg of a large fly, deposited in the cavity of the uterus after it had been laid open for examination.

Comparative anatomy is under great obligations to Sir Everard Home; but, on that account, we ought not to take his authority for more than it is worth, and most of all be on our guard lest we are misled by one who was extremely incautious and precipitate in forming his philosophical views. The Royal Society for the Promotion of Natural Knowledge has wisely always taken for its motto the words

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" NULLIUS IN VERBA."

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XXVIII.

OBSERVATIONS ON THE "AQUA BINELLI," WITH AN ACCOUNT OF SOME EXPERIMENTS MADE TO ELUCIDATE ITS SUPPOSED EFFECTS.

This water, for several years, has had some reputation in Italy, under the name of "Aqua Balsamica Arteriale," or the more brief one of "Aqua Binelli,"—the one designation derived from the properties attributed to it, the other from the individual who invented it.

The properties attributed to it by Binelli, and those who prepare and vend it at present, are not a little marvellous; such as the stopping both internal and external hemorrhages, and even of the large arteries when cut transversely—the cleansing and healing of all kinds of wounds—the renewal of uterine evacuations when suppressed, and the moderating of them when excessive,[1] &c.

My attention was called to this water first by the late Captain the Honourable Sir Robert Spencer, in the summer of 1830, after a

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[1] A pamphlet, descriptive of the effects of this water, distributed with it by its venders, thus opens:

"Da gran tempo è nota in Europa l' *Aqua Balsamica Arteriale* del dottor Fedele Binelli, nativo del Piemonte, e versatissimo in ogni meditazione ed esperimento di chimica et di bottanica. L' efficacia della medesima ad arrestare ogni più minacciosa effusione di sangue, fu mai sempre sì costante e di tal giovamento alla medica arte, che non potrebbe ottenersi più universale, e più meritata lode di quella che all' inventore di essa venne, da gran tempo, retributa."

visit which he had made to Naples, at a time when the extraordi-
nary virtues attributed to it were, in that city, a common topic of
conversation, and when the proprietors of the secret of its prepara-
tion were selling it in large quantities.

The specimen which I examined shortly after was part of a case
of several bottles which he had ordered to be forwarded to him at
Malta,[1] for the purpose of having its qualities fairly investigated.
It arrived after the decease of this enlightened officer, and was put
into my hands for trial in our military hospitals, by the then go-
vernor, the late Major-General the Honourable Sir Frederick Pon-
sonby.

I first examined into its physical and chemical qualities.   It
proved to be of the same specific gravity nearly as distilled water.
It was neither acid, alkaline, nor saline.   Its odour was not unlike
that of coal gas not purified, which it lost by boiling; the taste was
rather pungent; not in the slightest degree astringent.   In brief, it
appeared to be merely water containing a little volatile oil or naph-
tha, and was probably prepared by the distillation of water from
petroleum or some kind of tar.

I next made trial of it as a styptic.   I scratched the back of the
hand with the lancet till the blood flowed; the water, applied to
the scratch, rather increased the bleeding than stopped it.   The
following morning the razor inflicted a slight cut; the Aqua Binelli
was again applied, and the result was the same.

These few and simple trials were made in January, 1831, just
after I received the water, and they, of course, convinced me that
the thing was an imposition on the public, and deserving of no
further investigation.   About two years after, my attention was
recalled to the subject by a medical practitioner of Malta, who had
studied at Naples, inviting me, with others, to witness the effects of
a preparation made in imitation of the Aqua Binelli, and which, he
maintained, was identical with it in composition and virtues.

The experiment he invited us to witness appeared an unobjec-
tional one, namely, the partial division of the carotid artery of a
goat, the bleeding of which he undertook to stop by means of his
fluid.   He allowed us to expose the vessel and cut it across; about
one half of the circumference of the artery was divided, and the
bleeding was most profuse.   He stood ready with compresses,
moistened with the fluid, which he instantly applied, one over the
other, and secured them by rolling a bandage about the neck, mak-
ing moderate pressure on the wounded vessel; a little oozing of
blood followed, which soon ceased.   He said that in three hours
the bandage and compresses might be removed without any re-
newal of the hemorrhage.[2]

[1] The charge for this case was about 8*l.*   The price of the water at Naples
is four carlini an ounce, or 2*s.* 8*d.*
[2] The following marvellous result is related in the pamphlet already
alluded to,—witnessed, it is asserted, by a commission composed of some
of the most respectable medical men in Naples,—whose names are given,

Accordingly, at the end of three hours, they were removed, but, when the last compress was raised, the bleeding broke out as furiously as at first, and, to save the life of the animal, the artery was secured by ligature. On examining the last compress, a small coagulum of blood was found adhering to it, just the size proper to close the wound in the carotid,—thus accounting for the ceasing and renewal of the bleeding.

Reflecting on this result, and considering the chemical nature of the fluid employed to moisten the compresses, which appeared analogous to that of Binelli, the conclusion I arrived at was obvious, namely, that had the compresses used been moistened simply with common water, the effect would have been the same,—the bleeding would have been stopped ; and it also appeared very probable that had the compresses been allowed to remain undisturbed, there would have been no renewal of the bleeding.

To ascertain the truth of these inferences, the following experiments were made.

On the same day, February 8, 1833, in the presence of several medical officers, I divided transversely the carotid artery of two dogs; one small and feeble, the other of moderate size and strong. In each instance the bleeding was most profuse, till compresses dipped in common water had been applied and secured by a bandage, which, as in the case of the goat already given, completely stopped the hemorrhage.

The small dog, from the proportionally large quantity of blood which it lost, was very feeble immediately, and appeared to be dying; but it presently rallied, and for several days seemed to be doing well. It unexpectedly died on the 15th, seven days after the infliction of the wound. The bandage during this time had not been touched, and no application had been made. Now, on exposing the neck, the wound was found covered with coagulable lymph, discharging pus, and on dissecting out the artery and the eighth nerve contiguous to it, a mass of coagulable lymph appeared lying over the wound in the vessel, extending about half an inch above and below it. This mass of coagulable lymph having been carefully removed, and the artery slit open, the vessel was found quite pervious, not in the least contracted. The wound in the fibro-cellular tissue or external coat was closed by a minute portion of dense coagulable lymph. But not so in the middle and inner coat ; in these there was a gaping aperture, across which, on minute inspection, two fine threads, apparently of coagulable lymph, as if the

many of them professors in the university of that city. "Il Professore D. Nicola Mancini alla presenza della commissione, e di altri professori concorsi nel Teatro Anatomico del Regio Ospedale degl' Incurabili, apri trasversalmente l'arteria crurale di una pecora, vi pose sopra la filaccia bagnata nell' *Aqua Balsamica*, ed all' istante l'emorragia cessò; dopo un minuto fu tolta la filaccia, e l' arteria, con summa meraviglia degli astanti, si rinvenne già innestata, e la ferita netta come se mai da essa vi fosse sgorgato del sangue."

commencement of the healing process, were observable. The cause
of the dog's death was not discovered.

The other dog did not appear to suffer from the wound.   The
bandage and compresses were removed on the 15th February,
without the occurrence of any bleeding.   On the 20th of the same
month, the wound in the neck was nearly closed by granulations.
The artery was now cut down on, and the portion of it that had
been wounded taken out between two ligatures previously applied.
On careful examination of this excised part, it was found free from
coagulable lymph,—at least there was not the same thickening or
tumour from lymph deposited as in the former case; it was proba-
bly absorbed. When the external loose cellular tissue was dissected
off, a very minute elevation, about the size of a pin's head, appeared
on the site of the wound, the remains of the cicatrix externally.
The artery was completely pervious, and not at all contracted,
where it had been wounded.   Slit open for internal examination,
the wound in the inner coat was marked by a red line, interrupted
by two white spots; there was no gaping, the edges adhered to-
gether, excepting at one point, elsewhere the union was complete.
The white spots resembled the natural lining membrane, and had
the whole wound been similarly healed, I believe it would have
been impossible to have traced it.

These experiments were made in Malta.   I shall mention one
more, which was made after leaving that island, and subsequently
to the publication of an account of them in the Edinburgh Medical
and Surgical Journal.

On the 29th November, 1837, at Fort Pitt, the carotid artery of
a pointer was laid bare, and partially divided transversely.   Very
florid blood gushed out with great violence. Two linen compresses
dipped in water were immediately applied, one over the other, and
secured by a roller.   It was drawn too tight by an assistant, and
the hemorrhage was not suppressed until the pressure was relaxed;
indeed, it was necessary to remove the bandage, and re-apply it,
not tight.   About six ounces of blood were lost during the opera-
tion.   Nothing farther was done; there was no farther bleeding:
the dog's health was not affected.   In about a fortnight, the wound
in the integuments had completely healed; and in about six weeks,
namely, on the 12th of January, on examining the artery, carefully
dissected out after the death of the animal, from the injection of pus
into its pleura, no traces of the wound could be detected in the outer
coat of the vessel, or in the contiguous parts, nor internally when
slit open, excepting on narrow scrutiny, when a faint line was just
perceptible, marking the cicatrix, like the impression made by a fine
ligature on an artery not drawn sufficiently tight to divide the inner
and middle coats.

The general results of these experiments, (if I may be allowed to
speak so of so small a number,) are not without interest in applica-
tion to surgery.   They show how a hemorrhage from the wound

of a large artery, which by itself would be speedily fatal, may be
easily arrested by moderate compression, through the means merely
of several folds of linen or cotton moistened with water; and they
further show how, under this moderate compression, the wound in
the artery heals, the vessel remains pervious, and without the for-
mation of an aneurism; and how, after a time, the short space of
six weeks, only just perceptible traces of the wound are discover-
able.   Under this moderate compression, the healing of the wound-
ed artery seems to be very analogous to that of a wounded vein,
and apparently by means of the same natural process.

Whether similar results would be obtained were trial made of the
same means in the wounds of arteries in the human subject, can
only be ascertained positively by judicious experiments. The pro-
bability is, the results would be the same; the analogy is very com-
plete, and some facts well known in surgery accord with it, not to
mention the experience of the effects of the Aqua Binelli, as certified
by men of high respectability.[1]

I have laid stress on the effect of the *pressure*, afforded by the
wet compresses applied in the experiments related, believing that
the virtue of the means consists in the pressure,—of course not in
the water, excepting so far as it renders the compresses better fitted
for adaptation to the wound, to produce the degree of resistance
requisite to counteract the heart's impulse on the vessel; and also
better fitted to exclude atmospherical air. I would also lay stress
on the *moderate* degree of pressure that is produced in the manner
described, allowing the blood to pass through the canal of the
artery, and, as before observed, doing little more than resisting the
momentum of the blood in its passage from the moving source.
The importance of this moderate degree of pressure—reducing as
much as possible the wounded artery to the condition of a wounded
vein, is, if I do not deceive myself, very considerable. When I have
pressed with the fingers forcibly on the compresses applied to the
wound, expecting at the moment to arrest the bleeding, I have been
disappointed—it has continued. It has only ceased when the com-
presses have been secured, and not tightly, by a roller passed round
the neck of the animal. And further, in illustration, I may remark
that I have been equally disappointed in using graduated com-
presses, ensuring considerable pressure on the wound. This me-
thod has failed when general moderate pressure, effected by com-
presses about two inches long and one wide, succeeded. And, on

[1] It was my intention to have given a selection of the certified cases in
favour of the Aqua Binelli brought forward in the pamphlet which is fur-
nished with the water; but, on reconsidering them, it appeared a superfluous
labour, as the results, (giving them credit for correctness,) however excellent
in a curative point of view, are no more than the enlightened surgeon of the
present time may readily admit to be owing to water dressings alone, with-
out the aid of pressure; the majority of the instances adduced being exam-
ples of gun-shot wounds, and contused wounds, from which there was no
profuse bleeding, and no necessity, according to the ordinary mode of surgi-
cal treatment, for securing wounded vessels.

reflection, the difference of result is perhaps what might be expected ; the severe pressure can hardly arrest the bleeding except by pressing the sides of the vessel together, and closing the canal, the accomplishment of which requires a most nice adaptation, and a force which cannot easily be applied with steadiness except by mechanical means, and in situations affording firm support beneath.

Should the expectation which I have ventured to form of this method of stopping the bleeding of wounded arteries of a large size, in man, be realised on trial, I need not point out how very useful it may prove in military surgery, how very available it will be in the field and in battle, especially in great actions, when, however numerous and well appointed the medical staff of an army, the number of wounds requiring attention must always exceed the means of affording adequate surgical relief, according to the plan of treating them at present in use, of suppressing hemorrhage by ligature.

I have said nothing of the boasted efficacy of the Aqua Binelli given internally. I trust it is as little necessary to make any comment on it now-a-days, as on the tar-water of Bishop Berkeley, so very analogous in nature and reputation. Both the one and the other in some cases may be serviceable, but their principal recommendation seems to be, that in doubtful cases they are innocent.

XXIX.

ON A NEW METHOD OF PRESERVING ANATOMICAL PRE-PARATIONS FOR A LIMITED TIME.

During two years and a half, namely, between 1825 and the latter end of 1827, as leisure and opportunities permitted, I first made trial of a method of preserving anatomical preparations, which I believe to be new, and which I have found to possess advantages, in many respects, greatly exceeding the expectations I had formed of it.

Its principal advantages consist—first, in its cheapness ; secondly, in its durability within certain limits ; and, thirdly, in the clear and instructive manner in which it displays the minute structure of many textures and compound parts.

The cheapness of the method will be obvious, merely from the mention that the substance employed is a solution of the sulphurous acid gas in water, and that it may be prepared in a manner, equally economical and easy, by burning sulphur over distilled or rain water, in any appropriate glass vessel ;[1] agitating the water,

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[1] A tall receiver, provided with a cork, one-third full of water, answers well. The sulphur may be introduced kindled, in a small gallipot, attached

when the sulphur ceases to burn, and, when the water is suffi-
ciently impregnated with the acid gas, filtering the solution, to ren-
der it transparent and clear.

Its durability, to a certain extent, I infer from many circum-
stances. In December, 1827, when I communicated an account of
the effects of the acid to the Medico-Chirurgical Society of Edin-
burgh, in the third volume of whose Transactions it was published,
I had preparations by me, which had then been made nearly three
years—nothing further had been done to them—no fluid had been
added—no evaporation had taken place, and they then appeared
as perfect as when they were first immersed in the acid, and merely
confined in a bottle with a glass stopper, lubricated with a little
cerate. Other considerations induced me to think favourably of
its preservative powers. From experiments I had previously made,
the sulphurous acid appeared to have nearly the same power in
preventing the putrefaction of animal matter, as it has of stopping
the fermentation of vegetable juices. I found that serum coagu-
lated by this acid gas, and converted into a kind of jelly, may be
kept in water several weeks, exposed to the air, without undergo-
ing any change, and that the fibrin of the blood, thus treated, was
equally exempt from change. Similar trials were made with many
anatomical preparations; they were exposed to the air in this acid,
and were then taken out of the acid and placed in water exposed to
the air; and the result showed that after having been well acted
on by the acid, they were no longer liable to putrefaction. Some
of them underwent very little change, during exposure to the air,
for several weeks in water; others became soft and gelatinous, and
were partially reduced to a pulp, but without emitting any offensive
odour, like that of putrid animal matter, an odour rather like that
of decaying vegetable matter affected with mildew.[1]

How the sulphurous acid acts in preserving animal substances it
is difficult to say. I am not aware that it is yet ascertained how it
preserves vegetable substances; how it prevents vinous fermenta-
tion in vegetable juices, such as are most prone to undergo it, as
the juice of the grape, &c.; how it prevents light wines and vinous
fluids to which atmospheric air has access, from becoming sour or
converted into vinegar; or how, moreover, it stops the process of
ulterior change to which unpurified vinegar, and most, if not all, of
the unpurified vegetable acids are liable, when exposed to the air.

to a strong wire. As soon as the flame expires the sulphur should be quickly
withdrawn, the cork replaced, and the vessel agitated, that the water may
absorb the acid gas formed. After agitation for about half a minute, the
cork should be taken out—and fresh atmospheric air supplied by blowing
into the jar with a common bellows; the burning of the sulphur may then
be repeated, and the process so continued, until the water is sufficiently
strongly impregnated. Hard water should be avoided; for, if the water
contain any lime, it will render the solution turbid—a fault which even
filtering will not perfectly correct.
. [1] These experiments were made at Corfu, and during the hot summer
months.

It is not owing to the mere abstraction of oxygen, for phosphorus has not the same effect permanently, nor nitrous gas. It may be conjectured, both in the instance of vegetable and animal substances, that the antiseptic power of the acid depends on its effecting some new arrangements of the particles of the proximate principles which render them exempt from the changes alluded to ; or on rendering inert the vegetable and animal leaven which, under favourable circumstances, excite these changes.

The last advantage which I have mentioned, the manner in which the acid displays the minute structure of many textures and compound parts, is that which I shall most dwell on, as I consider it its chief recommendation to notice. It does not, like spirits of wine and a solution of alum, contract what is immersed in it; it does not, like a saturated solution of common salt, or of nitre, or of any of the salts of chlorine which I have tried, after a little while, lose its transparency and become thick and turbid ; nor does it, like a solution of corrosive sublimate, when used without precaution, deposit on the inside of the glass and on the preparation itself, a crust which soon becomes a complete mask. On the contrary, it expands and developes the parts, some more, some less, so as to magnify them and make them more distinct, effecting in structure what the lens does in vision ; and, at the same time, it remains clear, so that the lens still may be employed to heighten the effect and convey still more minute information of the object.

I shall enter into some details in illustration. All the descriptions will be taken from preparations from the human subject.

The skin immersed in the sulphurous acid swells considerably, and the cuticle is either thrown off or easily detached. The cuticle of the sole of the foot, to mention a particular instance, is rendered almost transparent, and is but little thickened. When held between the eye and the light, its symmetrical structure is beautifully shown, with innumerable dotted points, as it were, in the course of its linear waving, which probably serve the purpose of pores. If the cuticle, detached by means of sulphurous acid, is dried, it shows its peculiar structure still more distinctly and elegantly.

The cutis, distended by the sulphurous acid, appears as a tissue of extremely minute fibres and particles, condensed towards the outer surface, where in contact with the cuticle, and loose internally, where it gradually blends itself with the adipose texture.[1]

Serous membranes, immersed in the acid, swell considerably, and lose very little of their transparency. The inner surface of a portion of pericardium, now before me, has an uneven surface, and exhibits, when examined with a common lens, a slight appearance of pores ; its section shows as if it were composed of layers, and its outer surface displays a loose intertexture of fine fibres. A por-

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[1] In the scrotum and eye-lids—the transition is into cellular texture ; the surface of the cutis appears as the boundary merely of the cellular tissue a little condensed.

tion of pleura, (with the exception of the cut part, which is very much thinner, and not visibly in layers,) appears very similar, but less distinct.

On the cellular structure, the sulphurous acid has much the same effect as on the serous;—it distends it greatly, and preserves it transparent.

On fibrous membranes the sulphurous acid has not much effect; they become swollen in it slightly, and diaphanous. The outer layer of the dura mater, in a specimen before me, is considerably corrugated, whilst the inner has remained smooth, which, perhaps, is owing to the outer being of greater density than the inner, (as it appears to the eye to be); in consequence of which greater density, it is more expanded. The sclerotic coat of the eye is equally acted on throughout; and is not in the slightest degree wrinkled. The outer surface of the tunica albuginea testis is corrugated very slightly.

It acts more powerfully on tendons and ligaments; and on some more than on others, showing a striking difference in this respect. The tendons of muscles in general it expands amazingly, and renders semi-transparent. To take an example—the tendo-achillis thus swollen has the appearance of an opalescent mass, divided pretty regularly into cells, the boundaries of which are white opaque lines. On the ligamentous sheath of the penis its expansive power is less forcibly exerted; the sheath is rendered nearly transparent; and its structure is beautifully shown, as if composed of a white pulp as a ground-work, intersected and bound together by very fine opalescent fibres. On the aponeurotic sheaths of muscles its effects are very similar. On the chordæ tendineæ of the heart its effect is inconsiderable; it expands them but little, and renders them semi-transparent. On one part of the intervertebral substance it acts in the same manner as on tendon, of which part the inner portion, that nearest the cavity of the abdomen, chiefly consists, at least in the lumbar region; but, on the other part, its action is hardly perceptible. In consequence of this difference of action of the acid on the component parts of the intervertebral substance, it is well adapted to display its peculiar compound structure. When the acid is strong, an imperfect solution of a part of the tendon or ligament takes place, which subsides in the form of a semi-transparent very fine jelly.

Cartilage is not changed by the acid: neither the dense cartilages of the ribs, nor the delicate ones attached to certain organs, as the tarsus, epiglottis, &c. It is equally inactive on bone. Nor does it appear to have any sensible effect either on muscular fibre, or on the substance of the brain and spinal chord, or of the nerves and ganglia.

On mucous membranes its action is considerable: it does not appear to dissolve them in the slightest degree, but it distends them; renders them firm; and shows their peculiar structure in a

striking manner. As an example, I shall give a brief sketch of
the primæ viæ.

First, I may premise, that it displays the cuticle or epithelium,
terminating abruptly in the œsophagus, just above the entrance of
the latter into the stomach, and in the rectum, just within the cir-
cle of the anus, or the upper boundary of the sphincter muscle. In
both, the termination is distinct, and abrupt, and unquestionable,
as it appears in a preparation now before me.'

When the epithelium is removed, (and it is most easily, either
by placing it under a stream of water, or by the friction of the fin-
gers,) and the mucous coat is brought distinctly into view, in the
upper part of the œsophagus it appears very thin, provided with
many and pretty large mucous follicles on each side, and several
minute branches of nerves. With a magnifying glass of ordinary
power, it seems to contain a most delicate tissue of white lines,
running chiefly longitudinally, branching off slightly and anasto-
mosing. The same appearance presents itself in the middle and
lower part of the œsophagus, and more distinctly. But it is only
in the upper part that any nerves can be seen distinctly ramifying
in the semi-transparent mucous tissue. In the middle no follicles
are visible, and only a few near its termination. Where it termi-
nates, there is, in one preparation before me, quite a zone of what
appears to be minute follicles : but this does not show itself in ano-
ther preparation.

At the cardiac orifice of the stomach many minute follicles pre-
sent themselves. Where the gullet ends, and the stomach com-
mences, there is a sudden change of the appearance of the mucous
texture ; the linear marking suddenly ends ; and, as it were, a
dotted one commences, and, with some variations, extends through-
out this organ. When magnified, it has the appearance of a very
delicate lacework, formed in the upper part, by the close juxta-
position of circular lines ; and, in the lower, by lines in the form
nearly of the figure of 8; and, in the great arch of the stomach, by
lines irregularly tortuous. But this, it must be confessed, is not to
be seen in every preparation that I have examined ; and, therefore,
I would rather limit myself by saying, that, in the stomach, the
mucous texture has the appearance of a very delicate lacework of
lines or vessels.

At the commencement of the duodenum, the appearance of the
texture again distinctly changes. Here it appears as if covered
with tortuous threads or vessels twice or thrice the size of those of
the mucous coat of the stomach ; and, as the duodenum descends,

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¹ It appears very questionable, that the stomach and intestines generally,
the gall-bladder and the biliary ducts, the urinary, seminal and the air pass-
ages, possess an epithelium analogous to cuticle and a continuation of it.
All these parts must necessarily have a lining membrane, which seems to
be their mucous coat, in each part somewhat different. It has been sup-
posed that the mucus secreted by the membrane serves in lieu of cuticle,
and it seems to be a probable conjecture.

the appearance strengthens. In some preparations there is an ap-
pearance of follicles, and of circular depressions, but not in all I
have examined ; and, when seen, they are most distinctly seen with
the naked eye. A longitudinal section, including the pylorus,
strikingly and invariably shows a layer of a minute glandular
structure,[1] embracing and belonging to the mucous coat of the
duodenum, which terminates abruptly at the pylorus, but gradu-
ally in the contrary direction, and disappearing, or nearly so, about
an inch and a half from the pylorus. The same section, too, shows
how very thick the muscular and cellular coats of the stomach are,
in comparison with those of the duodenum.

The inner coat of the jejunum, viewed with the naked eye, in
sulphurous acid, appears to be distinctly villous, or as if studded
with innumerable projecting capillary points ; but this appearance
vanishes when it is examined with a common lens ; then, in some
lights, it appears to be covered with convoluted threads, having a
kind of centre, round which they are described, which, to the un-
aided eye, seems a minute mucous gland ; and, in other lights, as
minute projecting laminæ variously bent.

As the jejunum passes into the ileum, and as the ileum descends,
another change takes place, and it is very strongly marked close to
the termination of the latter intestine, where the mucous coat is
truly villous ;—so it appears to the naked eye in sulphurous acid ;
and, when examined in a favourable light with a lens, the villi ex-
hibit the appearance of tubular projections rounded at the ends ;
and some of them have a form approaching to the conical, rather
than the cylindrical. The sulphurous acid makes the glandular
structure[2] of the lower part of the ileum very distinct to the naked
eye.

This villous structure terminates suddenly at the valve of the
colon (where the valve begins,) and a new kind of structure suc-
ceeds it in the valve, and proceeds, with very little variation, of the
same kind throughout the large intestines, even to their termina-
tion, not excepting the appendicula vermiformis. It resembles
honey-comb in its appearance, more than any thing else to which I
can compare it ; but it is so very minute and delicate in every part
of the large intestines, that it cannot be well distinguished, except-
ing with the aid of a magnifying glass. The glands, on the con-
trary, of the large intestines, appear very distinct in the sulphurous
acid ; and their orifices are easily seen.

Relative to the other mucous membranes, I may briefly mention,
that this acid shows them equally as well as the primæ viæ, and
enables one equally well to notice how they differ from each other.

I shall now proceed to some other parts. By rendering the fib-
rous neurilema, and the connecting cellular tissue, semi-transparent,

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[1] The glands of Brunner, first described in 1687, and called by him, not
inaptly, *pancreas secundarium*.

[2] Glandulæ aggregatæ ; Peyer's glands, not discovered but minutely de-
scribed by him in 1677.

and by expanding these textures, the sulphurous acid has a remarkable effect in displaying the nerves, with the exception of the
ganglionic. It shows, almost without dissection, and still better
when aided by dissection, which it facilitates, the parts of each
fasciculus of nerves and their junctions; and, if the nerve be cut,
the proportion of the medullary matter which it contains, which becomes projecting, squeezed out, owing to the pressure just alluded
to. It shows very distinctly the fibrous nature of the optic nerves
beyond their junction; but, at their junction, and towards their
thalami, their substance appears to be exactly similar to the common medullary substance of the cerebrum. It shows the papillæ
of the tongue to be each the termination of a minute nerve, and
this without the aid of a magnifying glass. It demonstrates how,
as the nerves proceed from their source, the proportion of medullary matter diminishes, and the thickness of their sheath increases;
and this very remarkably in the nerves of the fingers. On the
ganglionic nerves, judging from the very few trials which I have
yet made, it appears to have but little effect, as if their composition
were different from that of the other nerves, as has been lately
maintained.

It displays, too, almost without dissection, the composition of the
vessels, and, I may add, of the passages and canals of the body
generally; owing to its expanding, in different degrees, the several
coats of which they consist.

The manner in which it exhibits the muscles is most distinct in
all the details of their anatomical structure. A transverse section
of a long muscle, well steeped in strong sulphurous acid, is an interesting anatomical object. With the naked eye its constituent
parts may be clearly seen, the blood vessels and nervous filaments,
the muscular fibres collected in bundles of different sizes, and connected by cellular tissue and ligamentous threads.

The effect of the acid on other compound parts is not less instructive. Were I to describe the appearance which the more important only exhibit in this fluid, I should greatly exceed the limits
to which it appears desirable to confine the description. In this
part of my subject I shall restrict myself to one example, which I
select, as being well adapted to illustrate the effects of the acid on
an organ variously compounded. This example is the penis: immersed in the acid, it swells like a sponge when put into water,
and very soon becomes completely distended; then, when the cutis
has been dissected off, its ligamentous sheath is brought beautifully
into view, with the nerves and vessels on the dorsum penis running
superficially through this sheath, now rendered transparent; and
sections, in different directions of the organ, display equally clearly
its internal structure, which, to the eye, either naked or assisted by
glasses of different powers, appears to be distinctly cellular, analogous to the substance of the lungs; or, perhaps, still more analogous in appearance to the structure of the common sponge, especially the anterior part of the corpora cavernosa, where the cells

irregularly formed are largest, and where, as in a cut piece of com-
mon sponge, there are large circular orifices, which seem to belong
to canals formed of ligamentous substance, which penetrate a cer-
tain way, and terminate abruptly with closed extremities.[1]

From such trials as I have yet made of the effects of sulphurous
acid on the textures of other animals besides those of man, I am of
opinion that it may be usefully employed in comparative anatomy,
especially to aid in the display of minute structure.   For this pur-
pose, inquirers will find it very serviceable, in the instances of pre-
parations sent from abroad unskilfully put up, or in examining the
viscera of animals which have been obtained entire, preserved in
spirits.   The effect of the acid in dilating the contracted parts,
especially those which are membranous and cellular, as the primæ
viæ, and the air passages and lungs, exceeds expectation.   If the
parts are delicate, I should mention, by way of precaution, that the
acid is not fitted for preserving them.   When distended, they should
be transferred to dilute spirit of wine.   If allowed to remain in the
acid, they may become softened, and in a few months reduced to
the state nearly of a pulp : this I have witnessed in the respiratory
and alimentary passages of small birds.

For the purposes of pathological anatomy, I believe the sulphur-
ous acid will prove equally, if not more useful, than for those already
alluded to.   I rest this opinion partly on the trials which I have
made, and partly on the consideration, that, whilst its expansive
power is so great on most tissues, its solvent power is very incon-
siderable, and on coagulable lymph appears to be null, (an ingredi-
ent of the blood, which, in most morbid changes, acts so important
a part) ; and it appears to be null on mucous membranes, the seat
of so many diseases.   The specimens which I have by me, of diseas-
ed parts preserved in this acid, are satisfactory beyond my expecta-
tion ; not so much as showing the exact diseased appearance, such
as it presented itself on dissection, as in giving a correct notion of
the nature of the lesion, by magnifying and rendering more distinct
the effects of the disease, contrary to what happens when the mor-
bid parts are immersed in spirits of wine.[2]

The sulphurous acid enables one to detect the slightest ulceration
of mucous membranes, existing even in so inconsiderable a degree,
that, without this aid, it might escape detection.   It enables one,
too, to detect the cicatrices of old ulcers ;[3] to detect coagulable lymph

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[1] These, probably, are the arteriæ helicinæ of Professor Müller.

[2] For the purpose of pathological research, the part to be examined may,
with advantage, be immersed for a few hours in sulphurous acid, and then
transferred to a cylindrical bottle, of thin and very clear glass, full of water,
perfectly clear ; very slight changes in the delicate structure of the part, be-
fore obscure, or not to be distinguished, will become apparent and distinct ;
and more particularly, very slight lesions and changes, whether recent or
old, of the mucous membranes.

[3] These cicatrices, from the observations which I have made, seem to dif-
fer chiefly from sound mucous membrane, in containing no follicles, when

poured out and adhering to their surface, even in the intestines, con-
stituting a false membrane ; and to notice many other changes in
relation to the different coats, vessels, &c. which it would be tedious
to enumerate.

It shows equally well the effects of disease on serous membrane,
on the blood-vessels, and on the viscera, especially the lungs.   I
have, in my possession, preserved in this acid, a very delicate speci-
men of tubercles on a portion of pleura costalis, covered with a very
fine net-work of coagulable lymph, and, after several months, it is
as distinct as when first immersed, and more distinct than before it
was introduced into the acid.   A portion of hepatised lung, thus
preserved, shows the air-cells filled with an albuminous deposition,
and the blood-vessels closed with the same, and the minute branches
of the bronchia.   And a vomica, which has been kept in this acid,
displays not only the ordinary phenomena of a tubercular excava-
tion. but, what is considered a very rare occurrence, blood-vessels
(veins) terminating in the cavity with open mouths.[1]

I apprehend it will prove very useful in studying the nature of
tubercles, and in determining some doubtful points respecting them.
There are two preparations now before me, which I may mention
in illustration ; one, a portion of lung containing tubercles in their
early granular stage ; another containing a tubercular mass ; the
former are clearly in the cellular structure of the organ ; in the latter
blood-vessels and bronchial tubes may be detected, clearly indicat-
ing that the tubercular matter has been deposited on them so as to
include them,[2] according to the analogy of the deposition of lymph
in hepatised lung.

The morbid changes of the arteries are very easily examined and
exposed by immersion in this acid, and chiefly in consequence of
its rendering the different coats so very distinct and distinguishable.
In all the specimens of aneurism which I have examined, and in
every instance of tendency to it, I have found the inner and middle
coat of the vessel diseased ; the latter generally diminished in thick-
ness, and rendered brittle, and sometimes entirely absorbed in the
dilated part of a true aneurism, or in the part contiguous to the
orifice communicating with the sac of the false ; and the inner coat
corresponding generally thickened, and corrugated, and easily
broken.   In many instances, since I have used the sulphurous acid,

the previous ulceration has been deep ; and, when only superficial, in exhibit-
ing a structure more or less different from that of the part in health.

[1] This occurrence, I believe, is much less rare than is commonly supposed.
It may frequently be detected in tubercular excavations, by passing fine
probes into the vessels which terminate in the cavity abruptly, marked by a
short projecting portion, their course having been interrupted by the ulcer-
ative process which formed the vomica. I have often found the canal of the
vessel unclosed, its aperture, in a manner, hid by the projecting portion,
which, in some instances, has been so situated as to act the part of a valve.

[2] The appearance mentioned above, not carefully examined, might give
rise to the idea, that tubercles are vascular.

I have noticed a very strong tendency in the arteries to aneurism, with only incipient dilatation, in which the middle coat of the vessel has been partly reduced to the thinness of the finest paper, and its colour changed to yellow ; and the inner membrane admitting of being easily detached, has been thickened to thrice or more its thickness in its sound state.

Lastly, in relation to the use of sulphurous acid for anatomical purposes, I must not conclude without offering some precautions taught by experience, the neglect of which may occasion want of success.

1. It is of consequence that the part to be preserved should be immersed in the acid, as soon as possible ; for if in the slightest degree putrid, it will not be well preserved, the fluid will become turbid, though frequently changed, and the preparation will not keep.

2. If the part be putrid, as is often the case, when taken from the body, though the *post-mortem* examination be made a few hours after death, it should be immersed in a solution of chlorine in salt and water, till deprived of its putrid smell and tendency ; then it may be washed clean, and put into the acid solution, without danger of spoiling.

3. Attention should be paid to the strength of the sulphurous solution, to proportion it to the nature of the part to be preserved, and the object in view in employing it. If the intention is to expand and develope the structure of the parts, for the purpose of demonstration, a strong solution answers best ; if merely to preserve a part as little changed as possible, a weak solution is most successful, especially if cuticle, the visible epithelium, or tendinous parts, are concerned, as a strong acid separates the former, and partially dissolves the latter.

When strong acid is thus used, if it be desirable to keep the part, the structure of which has been distended, it is safest to transfer it from the acid to dilute spirit of wine, as proof spirit diluted with twice its bulk of distilled water.

4. If the object is to preserve a part for a number of years, I would recommend in every instance the transferring it from the acid to dilute spirit, such as that just mentioned ; especially if the preparation be scarce, and of value in consequence. I am induced to offer this suggestion from the experience which I have had during the last twelve years : during this period some preparations have been well preserved ; others have undergone change, and have become spoilt. The uncertainty of preservative effect is sufficient ground for caution.

I believe, too, that this acid is not inapplicable to the service of the botanist, for preserving entire plants and their parts, especially the smaller and more delicate. I have a bottle now before me in which many kinds of vegetables are immersed, and have keen kept several months, and their forms are as distinct as ever, and in

excellent preservation; and amongst them I see the flower of the olive and the orange trees, and many more equally delicate.[1]

On the economical uses of this acid, and they are many, as in relation to its power of preventing the fermentation to which all the saccharine preparations of the kitchen are so subject,—of preventing the changing of wines into vinegar, to which all the lighter and more delicate ones are liable,—and, further, of preventing the common vegetable acids, as vinegar and lime juice, &c. and vegetable preparations in general, from spoiling, it would be unsuitable in this place to dwell. I shall merely remark, that the efficacy of the acid in all these respects is great and extraordinary, as must be known in part to many, and which may be learned by the easiest trials.[2] This subject, the economical uses of the sulphurous acid, is an important one to society, and would well reward the labours of the inquirer who would undertake the minute investigation of it.

[1] The effect of the sulphurous acid on the colour of vegetables, and I may add of animal substances, is much less than might be expected. Some vegetable colours it does not change, others but slightly, and very few entirely; and even the last mentioned return, though not with their original intensity, when the sulphurous acid is converted by the absorption of oxygen gas into the sulphuric. The red rose, bleached white by the fumes of burning sulphur, on exposure to the air, again becomes red. Of animal textures, those which are white, it renders delicately so; those which are yellow, as the gall-bladder, and common gall-duct stained by bile, the liver of this colour owing to disease, and parts of the supra-renal gland, it does not alter. It has very little effect in altering the healthy colour of the lungs or liver; but, where there is blood either effused in the parts, or contained in its vessels, the acid changes its colour to dark-brown, and, by coagulating it, fixes it for a time; and thus it is well adapted to detect the effusion of this fluid, in parts where it is apt to escape observation, as in the substance of the spleen, liver, &c. It is often difficult to distinguish between the redness produced by inflammation, and that which is occasioned after death by the staining power of the blood. I believe (and not hypothetically) that the sulphurous acid affords a means of distinction. If the change should have taken place after death, the part stained will be rendered brown by the acid, and in other respects appear sound; but if, during life, owing to inflammation, by immersion in the acid, either coagulable lymph will be detected effused on the surface, or under it, or serum in the adjoining cellular tissue, or some slight ulceration of the part affected.

[2] It is not improbable but that it may be useful for the preservation of meat, especially in a hot climate. I recollect, when sanguine of its efficacy, at the time I was engaged in experiments on it, keeping a chicken a week in the acid gas in the hottest time of the summer of the Ionian Islands, and having it approved of at table.

XXX.

ON THE ACTION OF VINEGAR ON ANIMAL TEXTURES.

Although vinegar, of all the acids, has been longest known, and from time immemorial used as a condiment, I am not acquainted with any account hitherto published of its effects on animal matter, minutely considered, or of its application to anatomical purposes. With the desire of obtaining such information, several years ago I instituted some experiments on the subject, the results of which I shall now record, believing that they may prove not without use or devoid of interest.

The vinegar I employed in the inquiry was distilled vinegar, almost colourless, of specific gravity 1006. This kind appeared preferable, both as belonging to the materia medica, and being nearly pure and almost always of the same strength. On the contrary, the common kinds of vinegar, those which have not been subjected to the process of distillation, are extremely various in composition ; and according to this variation,—according to the nature of the vegetable principles which they hold in solution, they are liable to vary in the changes which they themselves undergo, and in their effects on the animal textures immersed in them.

It might be expected, perhaps, that the solvent power of vinegar, on the textures of the body, would be considerable ; but, experiment proves it to be otherwise. In 1828, whilst stationed in Malta, portions of aorta, dura mater, brain, nerve, muscle, intestine, skin, liver, and intervertebral substance, were put into this acid, in a vessel not completely filled, and from which atmospheric air was not completely excluded. This was done in the month of January; at the end of six months, all the parts were in good preservation. The acid retained its transparency, but was slightly coloured, and there was a slight sediment. I may mention farther, to show how very inconsiderable is the solvent power of this acid, that on evaporation, it afforded only a very minute residue, of a light brown colour, semitransparent, viscid and bitter : the greater part of which was soluble in water.

The colourless acid produces very little change of colour in any of the textures on which I have tried it,—unless the parts were gorged with blood ; and then the alteration is referrible to the blood, the colouring matter of which, when not concentrated, is changed to brownish green ; and, when concentrated, to almost black.

To ascertain the degree of dilution, which the acid, of the specific gravity already mentioned, will bear, without being deprived of its antiseptic power, I have used it diluted with one, two, four, and eight parts of water. Portions of muscle were immersed in these mixtures of vinegar and water, and air was allowed to have access. Examined after two months, during which the atmospheric temperature varied between 80° and 92°, each portion of muscle was

found well preserved and free from putrid smell, excepting that in
the most dilute,—which, in a few days, began to putrefy: the ex-
periment was made in Malta. In this country I have made a few
other trials. In November, 1836, a portion of stomach, of duode-
num, ileum, rectum, and tongue, after having been steeped for a
day or two in distilled vinegar, were transferred to weaker acid,
(distilled vinegar diluted with four parts of water.) Three years
have now passed, and all these parts are in perfect preservation.
Each was placed in a jar apart, suspended in the fluid in the usual
manner, and covered with bladder, &c. to exclude atmospheric air ;
and, with the exception of one, after the lapse of the time mentioned,
no air has got admission. In the exception, the bladder was at-
tacked by insects, and yet, notwithstanding the accident, and partial
evaporation and free admission of atmospheric air, the preparation
(the tongue) is in a good condition, and displays well the peculiari-
ties of the organ.

Vinegar, I find, has not only the power of preventing putrefaction
from taking place, but also of arresting it, when commenced and in
full activity. So soon as the putrefying part is plunged into the
acid, the destructive change is stopped, and the part may be pre-
served without farther change ; and thus show the effect of putre-
faction on the texture in the stage in which it was arrested.

But, although the acid stops putrefaction, it does not, like the
sulphurous acid and corrosive sublimate, destroy the principle on
which the change depends, or effect an elementary change, incom-
patible with its taking place. I find that parts transferred from acid
to water, and well washed, undergo putrefaction under favourable
circumstances,—but much more slowly than if they had never been
in contact with the acid : and, the changes which take place in them,
seem to be, in some respects, different from those resulting from
ordinary putrefaction, and are, perhaps, peculiar.

In its action on different textures, in relation to its visible effects,
it very much resembles the sulphurous acid. Its expansive power
is similar ; its power of increasing the transparency of cellular tissue
and of the diaphanous membranes is also similar. And, in con-
sequence, like that acid, it is well fitted to show the structure of
many compound parts, and the minute structure of many organs. I
have mentioned preparations of different portions of the alimentary
canal, preserved for two years in dilute acid. Without the aid of a
lens, the peculiar structure of each division may now be distinctly
seen, and the mucous follicles even more distinctly than in the
instance of sulphurous acid,—in consequence of the glands them-
selves having acquired a certain degree of opacity, and the envelop-
ing tissue a certain degree of transparency. I have now before me
a thoracic duct, which has been kept in distilled vinegar nine years,
and it could not be shown better were it injected with mercury ; with
this advantage on the part of the acid,—that, whilst by distension it
shows the form of the vessel, it also, where the vessel is laid open,
shows its valvular structure.

From my own experience, I believe I am not too sanguine in expressing, confidently, my opinion, that both the anatomist and pathologist, engaged in research, may derive very material help from the use of this acid, and the latter especially, as it is well fitted to bring to light diseased structure, of such minuteness or obscurity, as to escape detection by the senses unaided.[1]

There are other uses to which this acid is applicable, some of which, as connected with my present subject, I shall briefly advert to.

On account of its antiseptic quality, I believe distilled vinegar may be often usefully employed in surgery, in washing foul ulcers, and in washing out abscesses deeply seated, containing offensive and putrefying matter. In one instance, of an abscess in the liver, which was opened by the knife, this acid, diluted with water, was employed, at my recommendation, and apparently with very good effect ; for, although air got admission, putridity was prevented.

Whether vinegar is efficacious, as a means of fumigation, for which purpose it is still commonly employed, it is not easy to decide. Its properties, on the whole, are rather unfavourable to its efficacy— at least permanently. However, as it ensures, by the pungency of its fumes thorough ventilation,—as it is most easily applied,—has no noxious quality that is known ;—and its activity, when concentrated, is certain and considerable,—perhaps it deserves the good opinion it has gained, and it may be right to continue the employment of it, as at present, when slight fumigation only is required, and there is no suspicion of the presence of any pestiferous fumes.

As vinegar dissolves tannin with facility, and in considerable quantity, it may be added, with good effect, in tanning such parts of the body as it may be thought desirable, by this process, to convert into dry preparations. The addition of the acid ensures the advantage of preventing putrefaction,—and the consequent destruction of delicate organisation. I have by me, thus tanned, the stomach with part of the œsophagus, its epithelium adhering, the valve of the colon and some other parts of the intestines. They display accurately, not only the forms and striking peculiarities of these organs, but also their delicate texture. To witness the latter, the parts should be saturated with water, by immersion for a few hours, and suspended in water in a cylindrical jar, to have the aid of magnifying power : in this way the villi of the ileum, even when tanned, may be made apparent.

Although vinegar has been so long known, and so long in use, and so very much and so variously employed, yet, I believe, that for culinary purposes it deserves to be more known and more employed, and, especially amongst the poorer classes, with a view to economy in the preservation of food. Amongst all classes, the waste

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[1] Minute ulcers in the intestines and loss of the villous structure, I have several times detected and made apparent by immersion in this acid, not perceptible in ordinary examination.

13—a

26 *dav*

of animal and vegetable food, fo.e not keeping, is great,—diminishing the quantity and enhanc g the price. Now, by means of vinegar, it is practicable to prevent this waste,—and it is most easily effected. In the hottest weather, during the summer in the Mediterranean, when cold meat became tainted and unfit for use in twenty-four hours, or even in a shorter time during the prevalency of the damp sirocco wind, I have had slices of roast and boiled meat, of fish, poultry, &c. put into vinegar, with a proportion of gravy or any plain sauce, and they were preserved perfectly good and fit for the table for weeks. It is well known how long vegetables may be thus kept. Fruit may be preserved in the same way ; and many kinds probably would be palatable and wholesome so preserved. In Asia Minor, and in the Ionian islands, the natives, as I have witnessed, are in the habit of keeping ripe grapes and ripe olives in vinegar,—and both are far from unpalatable ; the grape retains much of its sweetness, though a little disguised by the taste of the acid ; and the oil is preserved in the olive, free from rancidity, and of a very delicate flavour.

The formation of vinegar, naturally, in fruit and their juices, and in milk, seems as if it were intended, to insure their longer preservation for the use of animals as articles of food, as much so, as the changes beyond this, connected with decomposition and a new arrangement of the elements, appear destined to form manure and furnish food for plants.

XXXI.

EXPERIMENTS AND OBSERVATIONS ON THE PROPORTIONAL QUANTITY OF ANIMAL AND CALCAREOUS MATTER IN THE BONES OF DIFFERENT ANIMALS.

At distant intervals, my attention has been directed to the comparative composition of bone. The results which I have obtained, I shall now bring forward. Although they are far less numerous than I could wish, and too few for full and satisfactory comparison and induction, yet, I am not without hope, that as a contribution they may conduce to these ends,

The trials which I have instituted, have been of a simple kind, with a view to determine principally the proportion of calcareous and animal matter ; and this has been effected by calcination, or exposure to the action of fire in a crucible until the whole of the animal matter was consumed.

For the sake of brevity, I shall give the results in the form of tables, and I shall include in them the earliest which I obtained so long ago as 1811 ; and which Dr. Monro did me the honour of introducing into his " Elements of Anatomy." Indeed these early

experiments were made at his request, and principally on specimens which he was so obliging as to furnish me with for the purpose.

TABLE I.

Proportion of calcareous and animal matter in human bone, of different races, &c.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
1	Pars petrosa of the temporal bone of an adult (Scotish?) from Dr. Monro's museum: dry: how prepared not known	66·7	33·3
2	Temporal bone of the same, under the zygomatic process .	65·3	34·7
3	Occipital bone of the same .	60·0	40·0
4	A part of the lower jaw of the same between the symphysis and processes	59·5	40·5
5	Portion of parietal bone of another adult (British) from the same museum, its history unknown .	64·4	35·6
6	Parietal bone of another adult .	62·5	37·5
7	Shaft of tibia of an adult .	64·0	36·0
8	Shaft of thigh bone of an adult .	62·5	37·5
9	Occipital bone of an old man .	69·0	31·0
10	Part of lower jaw of an old person, between the symphysis and processes: alveoli absorbed: the bone brittle .	56·6	43·4
11	Occipital bone of a young person æt. about 15, recent, previous to drying .	58·0	42·0
12	Frontal bone of the same .	58·9	41·1
13	Parietal bone of the same .	58 8	41.2
14	Tibia of the same .	53.6	46·4
15	Fibula of the same .	44·0	56·0
16	Ileum of the same .	45·0	55·0
17	Thigh bone of the same .	47·0	53·0
18	Molar tooth of a man æt. 20, (sound) .	72·4	27·6
19	Ditto ditto 23, ditto .	75·0	25·0
20	Ditto ditto 30, ditto .	75·3	24·7
21	Ditto ditto 31, ditto .	73·1	26·9
22	Ditto ditto 32, ditto .	71·0	29·0
23	Portion of parietal bone of J. Keefe (Irish) æt. 33: died of phthisis pulmonalis, complicated with pneumathorax .	68·4	31·6
24	J. Spiers, Irish labourer, æt. 20: died of diabetes mellitus and phthisis pulmonalis[1] .	67·5	32·5
25	M. Bayley, English labourer, æt. 21: died of phthisis pulmonalis of very rapid progress .	67·2	32·8
26	J. Haliday, Irish labourer, æt. 29: died of phthisis .	68·1	31·9
27	G. Cocking, English labourer, æt. 21: died of phthisis .	66·6	33·4
28	T. Godfrey, English labourer, æt. 26: died of phthisis, complicated with pneumathorax of each pleura .	68·4	31·6
29	H. White, English labourer, æt. 20: died of phthisis of rapid progress .	67·9	32·1
30	E. Bridger, English labourer, æt. 21: died of phthisis of very rapid progress .	67·7	32·3

[1] When not specified, it is to be understood that the bone is of the same denomination as the preceding, and a similar part, and dried in the same way.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
31	D. Hurley, Irish labourer, æet. 30: died of devastating disease of knee joint, complicated with tubercles and small vomicæ in lungs	67·8	32·2
32	G. Mullins, Irish, æt. 30: died of complicated disease of lungs, partial emphysema, tubercles, &c. . .	68·0	32·0
33	R. Allan, Scotish blacksmith, æt. 20: died of phthisis, complicated with cynanche laryngæa . . .	66·9	33·1
34	J. M'Donough, Irish labourer, æt. 31: died of complicated organic disease without tubercles: softening of fornix: marks of old peritoneal inflammation: ascites, &c.	70·2	29·8
35	W. Macpherson, Scotish labourer, æt. 52: died of complicated disease, connected with an enlargement of heart and arch of aorta: partially ossified .	68·5	31·5
36	H. Higgins, English baker, æt. 45: died of suppurative inflammation of right elbow and ankle joint .	66·6	33·4
37	R. M'Cartney, Irish, æt. 28: cause of death obscure: probably syncope: it was sudden: a false aneurism, not ruptured, just below the arch of aorta . .	67·0	33·0
38	P. Kelly, Irish, æt. 48: died of complicated disease: softened fornix: chronic inflammation of bronchia	68·6	31·4
39	J. Masterton, Irish labourer, æt. 38: died of œdema of lungs, and thickening of small intestines after inflammation	68·5	31·5
40	J. Gray, English labourer, æt. 36: died of hydrothorax and œdema of lungs: (cranium subjected to coction without lime in the water, and it was afterwards greasy)	65·9	34·1
41	R. Stewart, Scotish, æt. 28: died of hepatised and œdematous lungs supervening on ulceration of tibiæ, enlargement of liver (it weighed 13 lbs. and contained no fatty matter)	69·4	30·6
42	C. Sidney, English, æt. 33: died from the bursting of a softened medullary tumour of the liver into cavity of abdomen	67·0	33·0
43	J. Shrub, English, æt. 42: died of hepatised lung, &c. complicated with a large aneurismal tumour, pressing on gullet and right bronchus . . .	63·5	36·5
44	J. Fairweather, Scotish labourer, æt. 34: died of a ruptured aneurism of the abdominal aorta (bone subjected to coction without lime in water; not apparently greasy)	68·4	31·6
45	J. Woods, Irish labourer, æt. 40: died shortly after disembarkation debilitated by sea scurvy (bone submitted to coction without lime, slightly greasy) .	64·4	35·6
46	W. Bickers, English weaver, æt. 44: died from the effects of sea scurvy (bone treated like the last) .	64·4	35·6
	T. Jones, æt. 49: died from the effects of sea scurvy[1]	66.7	33·3

[1] In these three cases the fatal event was probably owing to effusion into the lungs; in each there was œdema of these organs to a considerable extent, and besides this, no lesion in any part of the body of any consequence, in connection with the cause of death.

It is to be regretted that sea scurvy, which for many years was almost unknown amongst our invalids returning from our distant colonies, has lately become of not unfrequent occurrence, especially amongst old soldiers from

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
48	G. M'Carthey, Irish, æt. 31, maniacal: died of hepatisation and partial œdema of the lungs . . .	65·1	34·9
49	J. Brennan, æt. 34, imbecile: died of hepatisation of lungs	66·1	33·9
50	J. Lewis, English, æt. 41, maniacal: died of tubercular phthisis	65·5	34·5
51	J. Green, Irish weaver, æt. 25, imbecile: coma following convulsions and partial palsy preceded death; blood effused under dura mater, softening of fornix; partial hepatisation of lungs	66·9	33·1
52	J. Tomlinson, English, æt. 37, maniacal: died of partial hepatisation and gangrene of lungs . . .	68·5	31·5
53	J. Hanson, Dane, æt. 61, imbecile: died of malignant disease of orbit and liver (tumours) . . .	64·1	35·9
54	Anne Hartley, English, æt. 51, maniacal: died of cancer of stomach (medullary tumour) . . .	64·7	35·3
55	J. Ball, English labourer, æt. 42, maniacal: died of complicated disease with fibro-cartilaginous tumours of neck, compressing larynx and gullet . . .	64·9	35·1
56	Portion of frontal bone (of a very beautiful form) of a young Greek woman, of Ipsara, (it had been exposed to the air two years)	65·2	34·8
57	Portion of parietal bone, Albanian Greek, of middle age (how prepared not known) . . .	69·8	30·2
58	Portion of parietal bone of a Burmese woman, middle age (how prepared not known) . . .	66·5	33·5
59	Canadian Indian, middle age	64·6	35·4
60	Hindoo	63·7	36·3
61	Hindoo	65·8	34·2
62	Hindoo	64·9	35·1
63	Hindoo[1]	66·7	33·3
64	Hottentot, middle age	65·8	34·2
65	Bojesman	62·6	37.4
66	Ashantee	68·7	31·3
67	Ashantee	68·3	31·7
68	Ashantee	68·1	31·9
69	A portion of parietal bone of Ashantee . .	68·8	31·2
70	Ashantee	68·6	31·4
71	Ashantee[2]	69·3	30·7
72	Occipital bone of a negro (race unknown) from Dr. Monro's museum; very hard, compact, and white; dry, but not dried on stove	59·5	40·5
73	Occipital bone of another adult negro, from the same museum, less compact and hard . . .	58·9	41·1

India; and is to be attributed mainly to due attention not being paid to the health and comfort of the men on these long voyages; the provisions are often of bad or indifferent quality; lime juice is seldom supplied in sufficient quantity, or given in a judicious manner, and there is too often a neglect of attention to dryness and ventilation between decks.

[1] The four crania of Hindoos appeared to be of middle age; they had no marks of having suffered from exposure to the weather. They varied in size, form, and thickness; the weights of similar pieces cut out with the trephine were, 23·4 grs. 38·9, 49·6, 27·6.

[2] The Ashantee crania were taken from a field of battle, on which a large number of these people fell when they last invaded Sierra Leone;

In this first table of results, the specimens of bones from No. 1 to 17 were miscellaneous, from Dr. Monro's museum; they belonged, it is understood, to natives of Great Britain. Some were recent and moist; others dry, but not deprived of hygrometrical moisture before being subjected to trial. They afford proof, at least, that the proportions of calcareous and of animal matter in different bones of the same body are, as indeed might be expected à priori, somewhat different.

The specimens, the subjects of trial, extending from No. 18 to 71, were from the museum at Fort Pitt. Previous to incineration they were thoroughly dried, by exposure on a stove to a temperature of about 212°, till they ceased to lose weight. This is a circumstance of some importance in comparative experiments: if neglected, the results will be of little value for the purpose of comparison, whether the bones are recent, impregnated with their peculiar fluids, or dried by exposure to the air.[1]

The specimens from No. 23 to 55 were from crania prepared for the museum by the process of boiling, and in the majority of instances in water in which lime was suspended. To ascertain the

the exact time of their exposure is not known; probably it was less than a year.

[1] The variable proportion of the peculiar fluids of recent bones is, I believe, as great, if not greater, than of the solid contents: the recent parietal bones of Nos. 42 and 43 lost by drying, the former 18·49 per cent., the latter 23. The proportion of hygrometrical water is always changing in bone, as in other porous substances, according to the state of atmosphere in relation to humidity. I have found apparently dry bone lose by the expulsion of its hygrometrical water from 4 to 7 per cent. The power of absorbing moisture by different bones probably varies with their other qualities, as is witnessed in substances generally, and especially those used as articles of dress: I may offer an experimental illustration, and I am the more disposed to do so, as the subject has not received that attention which, practically considered, and especially in relation to health, it deserves.

On the 11th November, the substances which will presently be mentioned were weighed in a room in which was a fire, and where the thermometer stood at 60°, and a thermometer with its bulb moistened used as a hygrometer at 56; the weather was very damp; they were then taken to an alcove in the garden, where the dry thermometer was 50°, the moist 49°; after having been left there an hour, they were weighed on the spot; and next they were dried by the fire completely till all hygrometrical water was expelled, and weighed warm. The results were the following:—

	Room.	Alcove.	Fire-side.	Lost per cent.
Fine flannel	37·2 grs.	39·5	32·9	17·0
Brown merino	23·6	24·	20·5	14·6
Fine silk sarsenet	13·9	14·1	13·	7·8
Fine muslin	13·5	14·1	12 5	11·3
White kid glove	18·6	20·2	16·4	18·8
New linen	68.45	71·6	64·1	10·4
Old linen	53·	54·4	49·	9·9

The same substances were placed next the skin over the chest, where the thermometer was 89°, covered with a shirt, flannel waistcoat, &c.: after several hours they were found to weigh as follows:—Flannel 35·6, merino 22·25, silk 13·5, muslin 13·05, kid glove 17·5, new linen 66·2, old linen 50·9.

effects of the process, a portion of the parietal bone, No. 25, before being subjected to it, immediately after the post mortem examination, was deprived of its periosteum, and thoroughly dried and then incinerated; the residue of calcareous matter was 66·2 per cent. indicating 33·8 per cent. of animal matter, instead of 32·8 after the boiling, thus showing a removal of 1 per cent. of animal matter by the boiling process.

How the remaining specimens, excepting No. 56, were prepared is not known. They were sent from foreign stations, and were probably previously macerated in water, or left exposed to the air, like No. 56, till the soft parts had been destroyed by the action of the elements.

The crania extending from No. 23 to 55, it will be seen, were from four descriptions of patients:—1. those who had died of tubercular phthisis; 2. those who had died of other diseases not connected with tubercle; 3. those who had died of sea scurvy; and 4. insane persons. The majority of them were part of a series preserved during twelve months, taken from every fatal case that occurred, and put aside for the purpose of comparison.

The average proportion of calcareous matter afforded in these four different classes, was the following per cent.—

Phthisis - - - - -	67·7
Other diseases unconnected with tubercles -	67·6
Sea-scurvy - - - - -	65·1
Insanity - - - - -	67·5

With the exception of the instances of sea-scurvy, these differences are so small, that perhaps it may appear questionable that they are owing to disease: in sea-scurvy, it has been asserted that the bones have been affected, and that old fractures have become disunited. Three cases are hardly sufficient to inspire confidence in the conclusion. In each of the three, however, the proportion of calcareous matter being below the mean of all the others, is very favourable to the idea that in this disease there is actually an absorption of the earthy matter of the bones.

As the manner in which the crania of the foreign races submitted to trial were prepared is not known, no satisfactory inferences can be drawn relative to the proportions of the calcareous and animal matter in them. The results are so very different, especially in the African instances, that if of no other use, they are well adapted to prevent hasty generalisation.

TABLE II.

Proportion of calcareous and animal matter in bone in early and in advanced life.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
1	Portion of margin of parietal bone of a fœtus of six months	50.0	50·0
2	Portion of margin of parietal bone nine months old .	62·1	37·9
3	Portion of the same towards the centre . . .	63·4	36·6
4	Frontal bone of a child, the bregma still remaining, from Dr. Monro's museum	54·5	45·5
5	Parietal bone of the same	54·0	46·0
6	Lower jaw of a child from Dr. Monro's museum .	57.2	42·8
7	Milk teeth of a girl, shed at usual time, the roots absorbed: thoroughly dried, very brittle and fragile (three front ones weighed only 5·94 grs.) . .	79·5	20·5
8	Hollow summits of teeth of inferior jaw of a fœtus, between five and six months, several years preserved in spirit of wine, (they weighed only ·4 gr.) enamel not deposited	75·0	25·0
9	Under-jaw of the same, connected by ligament at symphysis, deprived of periosteum, nerve, &c. thoroughly dried, weighed only 8·5 grs. . . .	56.0	44·0
10	Portion of parietal bone of a Maltese woman, æt. 98: very thick and heavy, abounding in stearine .	47·6	52·4
11	Portion of parietal bone of a Maltese woman, æt. 83: unusually thick: it contained very little fatty matter	66·0	34·0
12	The occipital bone of an old man, from Dr. Monro's museum	69·0	31·0
13	A part of the lower jaw of an old person, between the symphysis and processes; the alveoli were absorbed, the bone brittle	56·6	43·4
14	Portion of radius from an old woman whose bones had been frequently broken, and were supposed to be peculiarly brittle, from Mr. Liston's museum .	66·0	34·0
15	Molar tooth of a man, æt. 61: weighed 31·9 grs. .	73·6	26·4

The bones noticed in this second table, with the exception of those from Dr. Monro's museum, were thoroughly dried previous to incineration. Both on account of the small number of specimens tried, and it not being known how they were all prepared, I shall not attempt to deduce any conclusions from the results: I have thought it right to give them, with the hope that they may excite doubt, and lead to further inquiry. There being no deficiency of calcareous matter in the bone No. 10, belonging to an old person who had repeatedly suffered from the accident of fracture of bone, would seem to indicate (supposing the tendency to fracture to be in the bone) that weakness and brittleness may arise from other circumstances than deficiency of phosphate of lime. This is a point deserving of special inquiry. Probably it will be found that in regard to strength and power of resistance, more depends on the arrangement of the constituent particles, and the proportion of fluid

matter interposed, than on the quantity of solid contents, especially the calcareous portion.

TABLE III.

Proportion of calcareous and animal matter in bone of different animals.

No	Description of Bone.	Calcar. Matter.	Animal Matter.
1	Portion of the tympanum of a whale, the species not determined: of specific gravity 224, and after having been subjected to the air-pump, 246 . . .	81·2	18·8
2	Pars petrosa of the temporal bone of another whale, species not known, from Dr. Monro' museum . . .	72·6	27·4
3	Pars petrosa of an elephant, from the same museum .	70·0	30·0
4	Under-jaw of another elephant	70·8	29·2
5	Under-jaw of hippopotamus	67·3	32·7
6	Under-jaw of dromedary	68·4	31·6
7	Metatarsal bone of a pure-bred horse, " Lorenzo," six years old, (just taken from the macerating tub) of specific gravity 1854, and after having been subjected to the air-pump, 2033: it lost by thorough drying 9·5 per cent.	65·77	34·23
8	Metacarpal bone of a troop horse, rather lower bred than usual, (prepared partly by macerating, partly by boiling) of specific gravity 2010 before action of air-pump, and 2077 after: it lost by thorough drying 5·45 . .	65·78	34·22
9	Shaft of humerus of a blood-horse (the compact part) before being subjected to air-pump of specific gravity 2045: after, 2092: it lost by thorough drying 7 per cent.	69·44	30·56
10	Similar part of shaft of humerus of a dray-horse, before being subjected to the air-pump of specific gravity 2000: after, 2126: it lost by thorough drying 6 per cent.	70·8	29·2
11	Parietal bone of Bombay buffalo, close to the horn . .	60·3	39·7
12	Horn of the same, near its base	3·02	96·98
13	Parietal bone of Persian goat	66·4	33·6
14	Ditto of Bengal antelope (goral) . . .	64·7	35·3
15	Ditto of damalis risea, Bengal . . .	66·5	33·5
16	Ditto of cervus capriolus	61·3	38·7
17	Ditto of sheep of Southern Africa . .	57·1	42·9
18	Extremity of process of parietal bone of the same, supporting the horn	57·6	42·4
19	Portion of horn of the same	2·7	97·3
20	Parietal bone of wild hog (Bengal) . . .	67·3	32·7
21	Superior portion of tusk of the same . . .	71·9	28·1
22	External table of parietal bone of another wild hog .	63·7	36·3
23	Parietal bone of tiger	65·4	34·6
24	Cellular structure of inner table of frontal bone of the same	65·6	34·4
25	Incisor tooth of the same	69·5	30·5
26	Molar tooth of the same	72·1	27·9
27	Lower jaw of common cat	71·5	28·5
28	Parietal bone of dog	68·2	31·8
29	Ditto jackall	68 4	31·6
30	Ditto wolf	70·5	29·5
31	Ditto hare	61·7	38·3
32	Shaft of tibia of tame rabbit	65·8	34·2

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
33	Shaft of femur of a young kangaroo	60·0	40·0
34	Ditto of mustela vulgaris, rather oily	66·6	33·4
35	Ditto putorius vulgaris	61·5	38·5
36	Parietal bone of full-grown seal (species unknown)	63·0	37·0
37	Ditto of another seal	68·4	31·6
38	Ditto of another seal	70·1	29·9
39	Shaft of femur of vespertilio vampyrus, Bengal	60·1	39·9
40	Ditto of vespertilio murinus, oily	57·0	43·0
41	Ditto of vespertilio noctula, oily	55·0	45·0
42	Ditto of radius of petaurus macrourus, oily and semi-transparent	62·8	37·2
43	Ditto of tibia and fibula of bathyergus capensis	63·0	37·0
44	Ditto of pteromys petaurista, oily and semi-transparent	62·0	38·0
45	Ditto of tibia of a common fowl (a hen)	68·6	31·4
46	Superior portion of sternum of the same	55·5	44·5
47	Ridge of sternum of wild swan of a delicate cellular structure	57·0	43·0
48	Portion of sternum of ostrich, including the delicate cellular structure connecting the outer and inner tables	56·6	43·4
49	Parietal bone of a large species of falcon	63·9	36·1
50	Parietal and frontal bone of pelican	63·3	36·7
51	Ditto of adjutant bird (ciconia argala) elegantly cellular	67·5	32·5
52	Ditto of buceros caratus	60·6	39·4
53	Portion of lower jaw of diomedea fuliginosa	57·2	42·8
54	Portion of spine of testudo græca	57·0	43·0
55	Sternal portion of the shell of the same	59·0	41·0
56	Portion of lower jaw of turtle of large size (Ascension—chelonia midas)	67·5	32·5
57	Ditto of gaviala tenuirostris	69·6	30·4
58	Ditto of trigonocephalus lanceolatus (St. Lucia)	70·1	29·9
59	Poison fang of the same[1]	62·1	37·9
60	Portion of upper jaw of the same	55·9	45·1
61	Shaft of femur of common frog	68·6	61·4
62	Ditto common toad; it was more oily than the preceding	58·1	41·9
63	Vertebræ of cod-fish (they lost by thorough drying 32·7 per cent.)	58·7	41·3
64	Portion of bony integument of ostracion quadricornis (it was dry, by thorough drying lost 5·7 per cent. hygrometic water)	40·4	59·6
65	Teeth of dog-fish (mustela lævis)	50·0	50·0
	Its skin yielded	24·0	76·0

All the bones noticed in this third table, excepting Nos. 2 and 3, were thoroughly dried previous to incineration; and the majority of them belonged to the museum at Fort Pitt.

The difference in the proportions of the animal and calcareous matter constituting them is less strongly marked than might have been expected; and, as it appears to me, not sufficiently great and constant in animals of the same natural families, to admit of any satisfactory conclusion being drawn on the subject. Nor is it pro-

[1] The canal in the fang of this snake was found to be in part cellular, its middle portion; after calcination, the part was very easily examined.

bable that any such conclusion can be drawn, until a series of comparative experiments shall have been instituted, conducted with minute attention to accuracy, and under circumstances, especially as regards the method of preparation, precisely similar.

It has been asserted that there is as marked difference in the proportions of the constituent parts of bone of the blood-horse and the cart or dray-horse, as there is in their appearance and some of their physical qualities; that the former, though stronger and more compact than that of the latter, is of lower specific gravity, and contains a less proportional quantity of calcareous matter. To endeavour to satisfy myself on these points, I have been at some trouble to procure for examination specimens of bone of the varieties of horse in question; for those of the thorough-bred horse "Lorenzo," and of a common life-guards' troop-horse, I am indebted to Mr. Gulliver. The results they have afforded are not in confirmation of the opinion referred to; they seem rather to favour a conjecture already proposed, that in relation to strength, more depends on the arrangement of the constituent particles than on the proportion of calcareous matter; and indeed many other of the results of the comparative trials contained in this table point the same way.

Table IV.

Proportion of calcareous and animal matter in exhumed and fossil bones.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
1	Portion of parietal bone of a human cranium, from an ancient tomb in Cerigo	73·8	26·2
2	Portion of zygomatic process of the temporal bone of an ancient Egyptian cranium from a tomb in Thebes; in form like the finest Grecian	76·1	23·9
3	Frontal Roman bone, found at Pompeii	64·5	35·5
4	Bone found at Borrowstownness, included in sandstone, apparently a human tibia	83·2	16·8
5	Bones from the banks of the Ohio, reddened by oxide of iron and penetrated by extraneous earthy matter, said to have been found at the depth of twelve feet . .	69·0	31·0
6	A tooth of the mammoth	69·5	30·5
7	The enamel of the same tooth	82·6	17·4
8	Portion of os humeri of a horse, found in clay near Upnor Castle, in Kent, twenty-four feet below the surface .	75·0	25·0
9	Portion of a cylindrical bone of an elephant, from a cavern in Corfu	95·0	5·0

For the few examples in this fourth table, I have been indebted to Dr. Monro, with the exception of Nos. 1, 2, 8, and 9. No. 1 I received through the kindness of Major Macphail, who, I understood, found it in an ancient Greek tomb in Cerigo, cut out of the porous calcareous free-stone of that island; and No. 2 was given me by the late Mr. Henry Stodart, who, on his return from Upper Egypt, assured me he had taken it himself from a tomb in ancient

Thebes. Both skulls are brittle; the former is probably more than
2000 years old, and the latter not less than 3000.

The considerable proportion of animal matter which these bones
contain, all but the last, is remarkably contrasted with the total or
almost total want of it in bones belonging to the most recent rock-
formations. In the bone breccia of the Mediterranean—so widely
scattered—I have been able to detect a just perceptible trace only
of animal matter: and in the teeth of the squali, which occur in
the tertiary formations of Malta and Gozo, I have not been able to
detect even a trace of it. In an enormous tooth[1] of one of these
fishes now in my possession I carefully sought for animal matter,
but in vain. They, and the fossil bones generally, which have not
been exposed to the air, owe their strength and hardness to a kind
of cement of carbonate of lime, which they all acquire. Judging
analogically from the partial effect of a known period of time, what
an idea of vast antiquity is conveyed by the circumstance of the
total destruction of the animal matter of bones! Perhaps the in-
stance of the Borrowstownness bone mentioned, in which the ani-
mal matter was only partially decomposed, may be considered as
in opposition to the preceding remark. But it hardly can fairly
be so considered, taking into account that it is probably human, as
its form indicates, and that the sandstone is probably of very recent
formation, and may be even still in progress.

TABLE V.

Proportion of calcareous and animal matter in diseased bones.

No.	Description of Bone.	Calcar. Matter	Animal Matter
1	The body of a large thigh-bone of an adult affected with hyperostosis, very thick and hard, but not very compact	76.9	23.1
2	The bone of a person supposed to have died of lues venerea, thick, light and spongy in a slight degree.[2]	70.7	29.3

[1] It is $6\frac{1}{4}$ inches long, 1 inch thick where of greatest thickness, and $4\frac{3}{4}$
wide where of greatest width.

[1] Death from lues venerea seems to be very problematical; and disease of
bones, as an effect of syphilis, seems also to be problematical. After an ex-
perience of nearly 25 years in the army, I do not recollect having witnessed
a fatal event consequent on lues venerea, fairly attributable to it alone.
When such an event has occurred more or less remotely following syphilis
—tubercles or other organic disease have been developed, and have been the
principal cause of death. And I have never met with any instance in which
in secondary syphilis the bones had been diseased, unless mercury had been
used. In offering these remarks, it is not my intention to maintain that
secondary syphilis is never complicated with disease of bones: it may be
so, and any other disease may be so complicated, accidentally, not essen-
tially—bone being liable to morbid action from a variety of causes, and
which may take effect during the progress of other diseases.

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
3	Cancelli of a curved tibia	74.5	25.5
4	The compact body of the same bone . . .	63.0	37.0
5	A portion of rickety parietal bone, about an inch thick	72.9	27.1
6	Another portion of the same bone . . .	69.5	30.5
7	The body of a rickety thigh bone, very thick . .	62.2	37.8
8	An exostosis	63.8	36.2
9	A scrofulous exostosis	63.0	37.0
10	A spinal process of a lumbar vertebra of the curved spine of a rickety person	59.3	40.7
11	A rib of the same person	59.2	40.8
12	The tibia of a rickety child, soft and spongy . .	26.0	74.0
13	A deformed female pelvis : soft and porous, and not unlike horn in appearance	24.2	75.8
14	Portion of detached necrosed bone	59.2	40.8
15	Callus of fractured femur of an adult, supposed to be very old : in appearance very dense—denser than the shaft	52.4	47.6
16	Shaft of the same bone, about two inches distant from the callus, (Fort Pitt Museum, Loc. Div. 4, No. 81.)	59.8	40.2
17	Callus of fractured femur of a person æt. 43. . .	50.4	49.6
18	Shaft of the same close to the callus, (Fort Pitt Museum, Loc. Div. 4, No. 65.)	55.8	44.2
19	Callus of another fractured long bone from the same museum, ...	61.2	38.8
20	Shaft of the same bone, which was more easily incinerated than the callus, ...	61.4	38.6
21	Callus of a fractured femur of a soldier, who died four years after the accident: very compact, .	62.8	37.2
22	Shaft of the same, about an inch from callus, (F. P. M. Loc. Div. 4, No. 19.)	61.2	38.8
23	Callus of a very old fractured femur, (its exact age not known) very compact, ...	58.3	41.7
24	Shaft of same, about 1 inch from callus, (F. P. M. Loc. Div. 4, No. 20.)	59.4	40.6
25	Fractured end of bone of leg of a rabbit, difficult to saw, 29 days after injury (from Mr. Gulliver.) .	65.4	34.6
26	Callus uniting the fractured ends of the same bone. .	54.6	45.4
27	Healthy portion of the shaft of the same, . . .	65.8	34.2
28	Exfoliation from human clavicle, presented to the museum by Dr. Williams, ...	58.0	42.0
29	Necrosed end of femur after amputation of limb, .	65.4	34.6
30	Portion of shaft of the same, near the end, . .	63.6	36.4
31	Portion of the same high up, ...	60.0	40.0
32	Necrosed shaft of femur, ...	60.6	39.4
33	New bone enveloping the necrosed shaft, (F. P. M. Loc. Div. 4, No. 22.)	57.1	42.9
34	Portion of necrosed tibia, near the head of the bone, its outer compact part, ...	63.4	36.6
35	Portion of new bone enveloping the preceding, (F. P. M. Loc. Div. 1, No. 95.)	43.6	56.4
36	Necrosed tibia of a boy æt. about 16, . . .	60.0	40.0
37	New bone belonging to the same limb,[1] (F. P. M. Loc. Div. 1, No. 169.)	53.6	46.4

[1] In this instance, as is generally, if not invariably the case, the new bone

No.	Description of Bone.	Calcar. Matter.	Animal Matter.
38	Porcelain-like deposit, (hard, compact, polished) on head of humerus, deprived of its cartilage, (F. P. M. Loc. Div. 2, No. 49.)	54.2	45.8
39	Portion of new bony matter deposited round the margin of the articular surface of the preceding, .	48.8	51.2
40	Portion of healthy shaft of the same bone: from a woman, who for many years had been subject to the gout, ...	58.8	41.2
41	Portion of tibia from G. Snellings, æt. 17: whose leg was amputated on account of caries with softening of ankle-joint, (F. P. M. Loc. Div. 2, No. 49.) .	38.2	61.8
42	Upper part of os humeri exhibiting osteo-sarcoma, so soft, as to be easily cut (F. P. M. Loc. Div. 2, No. 29.)	39.6	60.4
43	Tibia thickened and softened of an adult who had taken large quantities of mercury, (F. P. M. Loc. Div. 1, No. 33.) . . . :	60.0	40.0
44	Portion of os calcis from a limb amputated on account of disease resembling elephas: by drying, it lost 37.5 per cent.: it was soft, and abounded so much in oil, that it was semi-transparent,[1] .	15.0	85.0
45	Molar tooth of a soldier said to have been loose four years, ...	68.5	31.5

deposited to supply the place of the dead bone, showed no regularity of structure; after incineration, it was crushed to powder by moderate pressure.

[1] The limb was amputated by Mr. Shelly of Epsom, and by him presented to the museum at Fort Pitt, where it is now deposited. It would appear, from the information afforded by this gentleman—that his patient Thomas Grainger, an invalided artillery man, received a slight contusion on the dorsum of the left foot at the Cape of Good Hope, in 1806; that after apparent recovery, ulcers of a severe and intractable kind formed about the left ankle, for which he was sent home and discharged the service; that after deriving benefit from change of climate—he had a febrile attack in 1815, followed, whilst he was a patient in St. Thomas's Hospital, by swelling and ulceration of the foot; and with which he was discharged, the disease being considered incurable. In 1834, when Mr. Shelly first saw him, the leg had acquired an enormous size, and was discharging a very offensive sanies. He then objected to an operation. Ulceration continued and increased, the bones of the toes, it is stated, becoming carious and gradually disappearing. Life being in danger from the state of the limb, and from the hemorrhage which occasionally took place from the ulcerated parts, in the last week of August, 1838, he submitted to the operation of amputation, which was performed above the knee. The wound rapidly healed, excepting close to the ligatures; on the 21st October, nearly nine weeks after the removal of the limb, one ligature was still attached. The most remarkable appearances, which presented on examining the limb on its arrival at Fort Pitt, were the softened spongy state of the bones abounding in oil, (the tarsal and what remained of the metatarsal bones, actually floated when thrown into water;) a conversion of the muscles more or less into fatty matter; a condensed state of cellular tissue, with much accumulation of fat, and a hypertrophied state of the cutis and cuticle. In Ceylon many years ago, I had an opportunity of making a post-mortem examination of a native who died in the Leper-Hospital, of elephas, complicated with elephantiasis, (that is, the enlarged leg with the tubercular disease of the skin,) and the appearances of the affected

In comparing the results in this last Table, it appears how very variable is the proportion of calcareous matter in diseased bone; and what very little agreement there is between the quality of hardness and of softness of bone, and the proportions of calcareous and animal matter; further confirming the conjecture that more seems to depend, in relation to these qualities, on arrangement of the ingredients than on their respective proportions. Undue softness, it would appear, may co-exist either with animal matter in excess, which is the most common occurrence; or in about the average quantity, or even in deficiency, which is far more rare. Perhaps more extended research may lead to clearer general views of the connection of the proportions of calcareous matter with certain diseased states and physical conditions according to precise rules. But on this head there is little encouragement to be sanguine at present: the limited experience which we possess does not point to any such rules. The subject is one of difficulty, and of little promise, and therefore not likely to be prosecuted with the diligence and care requisite to discover order; and which assuredly can only be effected by the simultaneous study of the pathological state of the part, and its chemical composition, following it in its different stages.

In application to particular questions, the results contained in this table, I would hope, even limited as they are, may be of some use.

XXXII.

OBSERVATIONS ON THE CONTENTS OF THE URINARY BLADDER AFTER DEATH.

I have been induced to pay some attention to this subject, from finding, in an instance of diabetes complicated with tubercular phthisis, the urinary secretion, shortly before death, restored nearly to its healthy condition. The outline of the case was the following:—

J. Spiers, æt. 20, three weeks after enlistment was found to labour under diabetes. After treatment for nearly six months in the hospital of the regiment to which he belonged, he was sent to the general hospital at Fort Pitt, preparatory to being discharged the service. On admission, the symptoms of diabetes were well marked

limb were closely similar to the preceding. An account of the dissection is to be found in the last chapter of my work on the Interior of Ceylon. In this instance, the disease extended to the thigh: a large quantity of oil was found in the capsule of the knee-joint in the place of synovia. The bones were not examined.

and characteristic; those of phthisis, less so. The urinary secretion in the twenty-four hours amounted to six quarts, it was of sp. grav. 1010, and abounded in saccharine matter. He lived two months longer. Towards the termination of the disease the pectoral symptoms increased, the diabetic diminished; two days before his death, the urine was of sp. grav. 1017, and a few hours before of 1022, and was found to abound in urea, and to contain very little saccharine matter.

On inspection of the body, cavities and tubercles were detected in both lungs; and no other material organic lesion was discoverable.

The urinary bladder contained about one and a half pint of urine, of sp. grav. 1014, clear and of a bright brown hue. It was found on chemical examination to contain a large proportion of urea, without any appreciable quantity of saccharine matter.

Exclusive of this case, I have notes of twenty-seven others collected at Fort Pitt, in which, the inquiry, post-mortem, relative to the urine, was prosecuted. I shall give the results in a tabular form; stating the quantity of fluid found in the bladder; its appearance under the microscope when any wise peculiar; its specific gravity, ascertained by a delicate balance; its acidity (when possessed of that quality) by litmus paper; whether serous or albuminous, in a marked degree, or not, by the test of nitric acid; and the presence in it of urea, by the effect of the same acid on it, when concentrated by evaporation over boiling water, to the consistence of syrup. No selection of cases was made for the examination; almost every fatal case that occurred during a period of nearly six months was taken advantage of for the purpose; and I shall introduce the results obtained from each, in the order of time, rather than classed according to the nature of the fatal disease—this being most in accordance with the experimental plan.

TABLE

Of observations on the Urinary Bladder and urine after death.

No.	Age.	Principal organic lesions discovered by autopsia.	Hours after death.?	Observations on the urine.
1	51	Cancer of stomach (medullary tumour softening, with ulceration)	17	About 8 oz. found in the bladder: ale-coloured: of sp. gr. 1015: not serous: abounding in urea
2	33	Tubercles and cavities in lungs, with pneumathorax from perforation of pleura of right lung	11	About 1 dram of urine in bladder: of natural colour and appearance
3	58	Partial gangrene of left lung: 12 pints of serum in cavity of abdomen: peritonæum thickened and granular	6	About 4 oz.: of natural brownish hue, with slight mucous sediment: sp. gr. 1013: not serous: abounding in urea
4	21	Tubercles and cavities in lungs, &c.	9	Bladder quite empty

No.	Age.	Principal organic lesions discovered by Autopsia.	Hours after death.	Observations on the urine.
5		Tubercles and cavities in lungs, &c.	31	1 dram : turbid : serous
6	25	Ditto ditto	16	6 oz. : porter-coloured : sp. gr. 1019 : abounding in urea
7	42	Partial hepatisation of lungs : a large aneurism of thoracic aorta, not ruptured	12	A few drops only : too little for trial
8	28	Tubercles and cavities in lungs; a medullary tumour in lower portion of spinal canal. Ureters and urinary bladder bloodshot, &c.	23	½ oz. : turbid, purulent : of sp. gr. 1027 : under microscope pus-globules seen in it, with a few blood-corpuscles : and particles of a smaller size : no urea detected
9	40	Partial œdema of lungs : gangrenous fistula in ano	22	3 oz. : ale-coloured : sp. gr. 1011 : slightly serous : abounding in urea
10	31	Tubercles in lungs with cavities in left lung	6	1 oz. : ale-coloured : sp. gr. 1014 : not serous : urea not deficient
11	25	Hepatisation and œdema of lungs	21	4 oz. : porter-coloured : of offensive cadaverous odour : contained pus-like globules, and many spermatic animalcules : sp. gr. 1020 : not serous : abounding in urea
12	22	Ecchymosis with bloody serum in lungs, containing pus-like globules ; rima glottidis, slightly œdematous : valves of heart and large blood vessels strongly stained by colouring matter of blood : pus-like globules in blood, and in softened subst. of spleen[1]	12	2 oz. : ale-coloured : odour very offensive : sp. gr. 1317 : abounded in urea
13	29	Cavities in left lung : tubercles in both	28	3 oz. : porter-coloured : odour offensive, cadaverous : acid : sp. gr. 1019 : not serous : a few pus-like globules in it, and many much smaller particles
14	44	Hepatisation and œdema, with partial gangrene of lungs	28	3 oz. : porter-coloured : acid : slightly serous : sp. gr. 1023 : one spermatic animalcule detected : a few pus-like globules, and fine filaments : abounding in urea
15	39	Cavities and tubercles in lungs	28	1 oz. : high-coloured : odour offensive : sp. gr. 1022 : slightly serous : not deficient in urea

[1] This case of confluent variola which proved fatal on the 14th day, on post-mortem examination afforded a striking example of red discoloration or staining of lining membrane of the heart and great blood vessels, in connection with red serum and incipient putrefaction ; the blood mixed with lime emitted an unusually strong ammoniacal odour, and the blood particles were puckered and diminished in size. At page 103, will be found some observations on the dependence of the staining of the heart and great vessels, on the cause just referred, viz. putrefaction and the serum of the blood holding in solution colouring matter of the blood.

No.	Age.	Principal organic lesions discovered by Autopsia.	Hours after death.	Observations on the urine.
16	33	Cavities and tubercles in lungs	20	No urine in bladder
17	33	Caries with partial necrosis of frontal and parietal bones: softening of fornix: tubercles and cavities in lungs	17	4 oz.: ale-coloured: sp. gr. 1006.
18	21	Cavities and tubercles in lungs	21	2 oz.: porter-coloured: not serous: sp. gr. 1017
19	35	Lungs partly œdematous, partly hepatised, and containing puriloid fluid. Puriloid matter and fibrinous concretions, obstructing iliac veins, connected with sea scurvy	28	A few drops only
20	47	Ulcers in rectum and lower portion of colon: a perforation into cavity of abdomen, with gangrene and marks of peritoneal inflammation	15	3 oz.: ale-coloured: acid: slightly serous: sp. gr. 1015
21	18	70 oz. of serum of sp. gr. 1019 in left pleura: 27 in right of the same sp. gr. 3½ oz. in pericardium of sp. gr. 1014: tubercles in both lungs and cavities in the left	37	Slightly serous: acid: sp. gr. 1016; abounded in urea: contained pus-like globules and many very minute animalcules—thread-like—their length equal to about the diameter of a blood-disc, some with a vibratory, others an undulatory motion
22	30	Tubercles and cavities in lungs	33	4 oz.: straw-coloured: slightly turbid: not serous: contained spermatozoa and some pus-like globules: sp. gr. 1013: abounded in urea
23	38	Lymph extensively effused over pericranium (erysipelas): two small cavities and a few clustered tubercles in lungs (latent phthisis)	6	1½ oz.: dark ale-coloured: acid: serous: sp. gr. 1021: very little urea: many globular filamentous particles, probably of mucus, and deriving their form from mucous follicles
24	31	Tubercles and cavities in lungs	25	1 oz: high-coloured: acid: not serous; sp. gr. 1019: not deficient in urea: contained many minute particles: one spermatic animalcule was seen, and a globular animalcule in motion, smaller than a blood corpuscle
25	39	Tubercles and cavities in lungs. Large intestines several ulcerated: end of appendicula vermiformis perforated by ulceration, adhering to colon, and gangrenous where adhering	13	1½ oz.: light ale-coloured: slightly turbid: acid: sp. gr. 1014: slightly serous: contained many spermatozoa, and very minute particles: not deficient in urea
26	33	Ulceration of pharynx and larynx, with œdema of margin of epiglottis and glottis, and partially of the lungs, with destruction of a considerable portion of the palate, including the greater part of the bony plates	34	4 oz.: pale ale-coloured: a mucous sediment on standing: sp. gr. 1027: acid: slightly serous: contained some urea

No.	Age	Principal organic lesions discovered by Autopsia.	Hours after death.	Observations on the urine.
27	33	Tubercles and cavities in lungs	29	1 oz.: ale-coloured: sp. gr. 1020: acid: slightly serous: contained urea
28	35	Fracture of cranium, with laceration of middle meningeal artery and extravasation of a large quantity of blood within the cranium	27	1 dram: a sediment of gelatinous mucus, consisting (as seen under the microscope) of particles, globular and filamentous, like those noticed in case 23. The little supernatant fluid was acid, and resembled some urine, drawn off by catheter, ten hours before death, of sp. gr. 1010: and abounding in urea

Do not these observations seem to indicate that the kidneys, in the majority of cases, perform their function even in the last hours of life; and that the secreted fluid of the moribund differs less from healthy urine, than in many instances of diseases, not threatening life? The first case, introductory to the others—that of diabetes complicated with phthisis, is a striking example in point. Admitting the inference to be correct, it is necessarily matter of conjecture how the effect takes place. Other organs exhibit phenomena somewhat similar. In phthisis, shortly before death, it is not uncommon to witness a cessation of distressing pectoral symptoms, and a return of comparatively easy respiration. In insanity, complicated with organic disease of either the thoracic or abdominal viscera, and especially with tubercular disease, it generally happens that the destructive malady runs its course, without apparently deranging the functions of the organs in which it is seated. In all organic diseases, and most distinctly marked in instances of chronic disease of the heart, there are marked periods of rest, as well as of exacerbation. And during the fits of aggravation, certain medicines, as narcotics, timely administered, often have the effect of affording great and sudden relief, seemingly allaying the deranged functions and restoring the organ to its healthy state of action. Lastly, it may be remarked, that the functions of most organs, even when free from organic disease, are liable to irregularities; as the sudden blush, the quickening of the pulse, the opening of the pores and bursting forth of sweat, the sudden change of the urinary secretion, becoming at once excessive in quantity and extremely dilute—all connected with mental emotion. Now, as in these instances the modifying effect appears to depend on the nervous system exerting some peculiar influence, it seems not improbable, that, in the last stage of life, a similar influence may be exerted on the kidneys, and a kind of healthy action temporarily restored. But this is speculation—and as such of little weight. Practically considered, perhaps a useful caution may be deduced, from the comparatively healthy state of the urine in death; a

caution of not attaching too much importance to the condition of the secretion during life, and in instances of disease, especially on casual observation and trial. The natural inference seems to be, if in dying the urine can resume its healthy qualities, or an approximation to them, it is likely to do the same occasionally, under peculiar influences, during the prevalency of active disease. Examples of this kind are probably familiar to every experienced practitioner, who has directed his attention to the subject. And the propriety of using caution in this matter seems to be enforced by what is now and then witnessed in the condition of the secretion, in persons apparently in perfect health—a condition the reverse of that alluded to in death, viz. a change from the healthy to a morbid state—as a sudden and unexpected deposit of a large quantity of brick-red or rose-coloured sediment, to the no little alarm of the individuals, till they find (as often happens) that such a deposit is of as little consequence as the appearance of a pimple on the forehead, or of a papular eruption on the lip in sound or apparently sound health.

Reverting to the particular observations, it appears, that in two instances—cases No. 21 and 24, minute living animalcules were seen in the urine under microscopic examination. I believe they came from the bladder; but it is right to state I am not certain that they did, as the vials in which the fluid was received had previously held pump-water. In using the microscope it is impossible to proceed with too much caution, or to be too sceptical, especially when the results are in any wise extraordinary.

Further, reverting to them, it appears, that in several instances, viz. in cases Nos. 11, 14, 22, 24, and 25, spermatic animalcules were detected in the urine. Of the nature of these animalcules, there could be no question, their forms were so distinct. Nor can there be, I apprehend, any doubt entertained that they had been collected previously in the vesiculæ seminales. Probably in the convulsion of death, which is often accompanied with the discharge of a portion of the contents of these organs, some of the fluid may have been irregularly propelled into the urinary bladder.

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XXXIII.

SOME DIRECTIONS FOR MAKING AND KEEPING ANATOMICAL PREPARATIONS IN HOT CLIMATES.

It is too generally supposed that the making and keeping of anatomical preparations in hot climates is almost impossible, or attended with so much difficulty as to be practically impossible, with the ordinary means within the reach of medical men.

This is a mistaken notion, and it is for the interest of science that it should be refuted.[1] The changes which animal matter undergoes at a temperature between 80 and 90° Fahrenheit (the average maximum of the highest temperature, in the hottest seasons, even in intertropical climates,) do not differ in *kind* from those which occur at a temperature between 45° and 55°, and *à fortiori* between 55° and 70°, which may be considered about the average in-door temperature of the winter and summer seasons in Great Britain. The difference then in the changes is chiefly in *degree :* in a hot climate they take place more rapidly, than in a temperate one, twice or thrice as rapidly, according to the elevation of temperature. This should always be kept in mind as a maxim and principle ; and to ensure success in making anatomical preparations, the rapidity of change of animal matter must be met with proportional quickness and energy of the conservative processes of art opposed to the destructive ones of nature. With the same view, and against the same tendency to change and decompose, besides quickness, great neatness and cleanliness are requisite.

Every dissection should be conducted in a regular and scientific manner, according to a certain method, and with definite objects in view. The principal objects of all dissection are three : the detection of the effects of the disease, and the cause of death ; the removal of diseased parts for preservation ; and the acquisition of general anatomical knowledge. Neatness, and cleanliness, and method, conduce equally to these objects. Attending to them, obscurity, confusion, and error are avoided ; the pursuit loses as much as possible its disgusting aspect; it gives information of a satisfactory kind ; excites interest powerfully ; and zealously pursued becomes almost fascinating. Farther, when the dissection is conducted on these principles, it is the source of much valuable instruction. It makes the hand dexterous for surgical operations ; it produces caution in deciding on *post mortem* appearances, which are so often deceptive ; and habituates the eye to the nice discrimination of what is sound in structure, and what is diseased.

When any morbid appearance presents itself, the part displaying it should be carefully examined before it is removed ; its situation

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[1] My attention was directed to the above subject in 1825, when the present director-general of the army medical department was calling on medical officers on foreign stations to contribute to the museum at Fort Pitt, then in its infancy, and which now, owing in great measure to the assistance so afforded, is become one of the most valuable collections of pathological anatomy in this or in any other country. Most of the observations contained in this paper were written at that time, and privately circulated with the view to remove the impression alluded to in the text; and were afterwards published in the 8th vol. of the Edinburgh Medical and Surgical Journal. I am induced again to bring them forward, with such alterations as extended experience has suggested, with the hope that they may be still useful in the important cause of pathological research, and of that museum which is the permanent record of such research amongst the medical officers of the army, by whom it has been formed.

should be noticed, and its connections traced. If it is considered worthy of being preserved, with that intention, it should be dissected out so as to appear to the best advantage; to require as little explanation as possible; and to be by itself as intelligible as possible.

If dissected out neatly and cleanly at once, free from extraneous, adipose, cellular, and muscular substance, &c. much subsequent trouble will be spared, and time saved. Generally speaking, indeed, the suggestion just given cannot be too strongly inculcated. For the morbid part to become a good preparation, it should be put out of hand at once; and nothing should be left that ought and can be removed by the knife and scissors. Delay breeds neglect and forgetfulness; the nicer peculiarities of the diseased part are forgotten; after a time it ceases to excite interest, as a confused mass putrefying, or bordering on putrefaction, it is a loathsome and worthless object; and thus, in consequence of not having been finished at once, it ends in being thrown away; and an aversion too often is acquired, rather than a fondness for pathological anatomy.

The present remarks are applicable to all kinds of anatomical preparations, but more especially those intended to be kept in spirit of wine, which experience has proved is better adapted for the preservation of moist preparations than any other liquid yet tried.

The methods of proceeding in preserving preparations, must to a certain extent be modified according to the nature of the morbid parts, and agreeably to the intention of the anatomist.

If the diseased part is small, and it is wished to preserve its colour, as a portion of inflamed, or of ulcerated stomach or intestine, it should be immersed immediately in strong spirit; and instantly put up, as it is intended it should remain. After a month the spirit may be changed for fresh spirit, and the mouth of the vessel should be firmly secured. The blood in the part will thus be coagulated and preserved; the shape will be retained without unseemly distortions, which when once rigid are not easily removed; and the preparation is fit for the shelf of the museum without any further trouble.

Preparations of the brain, spinal cord, and nerves, should be treated in the same manner, and so treated they are easily kept.

Thus also should be managed preparations of the eye, pleuræ, peritoneum, testes, and their tunics, and in fact all such parts as are either colourless, and therefore not requiring steeping in water, or are liable to be injured by that process, and by incipient putrefaction.

On the contrary, parts containing much blood, as the liver, kidneys, lungs, heart; or stained and discoloured with blood or bile, &c. as the blood vessels and gall-bladder, &c.; or, smeared with a lubricating fluid, as the aspera arteria, primæ viæ, synovial membranes, &c. should be immediately well washed, and if practicable, in a stream of water. By persevering washing in *running* water, and gently pressing the part, treating it like a sponge, in a

short time, as from ten to twenty minutes, most textures may be
deprived of the blood which they contain, and of any secreted
fluids peculiar to them.   If leisure does not admit of such manipu-
lation, and assistance is not available for the purpose, the parts
may be suspended in water, and the hotter the weather is, the
shorter should be the time of suspension, and the more frequently
the water should be changed.   In thus steeping, it is of importance
to use a tall large vessel, full of water—and to have the parts im-
mersed only just below the surface.   The rationale of this must
be manifest to those who reflect on the subject, and take into con-
sideration, 1st, that blood and bile, especially the colouring matter
of the former, is of greater specific gravity than water, and conse-
quently has a tendency to subside rapidly and collect at the bottom ;
and 2dly, that the colouring matter of both, and more particularly
of blood, in solution in water, has the power of combining with
animal textures, and of staining or dyeing them.   By using a tall
vessel in the manner suggested, the colouring matter in dissolving,
will rapidly quit the part immersed, and descend beneath it ; and
by employing a large quantity of water, the object of separation is
more effectually attained, and very frequent change of water is so
much the less necessary.   Demonstrative proof may be easily ob-
tained of the propriety of these directions, by reversing them, as by
allowing the part in steeping to sink to the bottom, or by using a
shallow vessel or a small quantity of water.   In the former in-
stance, the part will necessarily be immersed in a strong solution of
the colouring matter, and will be intensely dyed ; and in the latter,
the effect will be very similar, presuming that the colouring matter
is diffused through the whole of the water.[1]   I may appear to lay
too much stress upon these points : but, in reality, they are very
important, and commonly not sufficiently attended to.   I have
known them neglected even by anatomists, not unaccomplished in
the art of making preparations for the museum ; and I also know
how difficult it is to have them observed by novices in the art, for
whom chiefly these observations are intended.

Immediately after the thorough washing, as soon as possible, if
steeping be employed, the parts should be transferred from water
to spirit. Before immersion in the latter, unless of peculiar delicacy,
they may be pressed gently between folds of linen, to remove the
excess of water.   A mixture of seventy parts of proof spirit, and
thirty of water (rain, or distilled water should be used[2]), is well

[1] Vide note, page 104, for further remarks on the staining agency of
the colouring matter of blood.   Whether dissolved in consequence of inci-
pient putrefaction in serum, or merely in water previous to putrefaction even
in an incipient state, its dyeing power appears to be similar.

[2] If the water contain carbonate and sulphate of lime, which are of com-
mon occurrence in spring water, they will be precipitated by the spirits ; the
appearance of the preparation may thus be injured, or the inside of the jar
may become dull, and prevent a distinct view of the contents.

adapted, even in warm climates, for preserving the majority of
moist preparations.

For the useful purposes of a museum, it is necessary that the
part to be kept should be, not only carefully and neatly dissected
out, but also carefully and neatly put up, and *that immediately*,
and as it is intended it should appear on the shelf. If this be
neglected at the moment, the season for doing it in perfection is
lost. A preparation crammed into a bottle, just large enough to
hold it, or thrown into spirits in a large vessel, as is too often done,
without attention to suspending it in a natural way, that it may be
properly seen, becomes (unless it be some very simple structure)
misshapen, distorted, and confused. A preparation in such a state
it is very difficult afterwards to amend : this is well known to those
who have had occasion to attempt the annoying task of endeavour-
ing "to make something" of a preparation, perhaps highly inte-
resting in itself, which has been thus neglected in the first instance.

It may perpaps be thought that the measure just recommended
is not easily carried into effect; that to accomplish it much art and
skill are requisite; and that glass vessels in plenty, and of various
sizes, are indispensable.

This, fortunately, is not the case; very moderate skill is suffi-
cient, such as every medical man ought to possess, and must pos-
sess, if he is fond of his profession, and only tolerably zealous in the
pursuit of it. And instead of many glass vessels, one or two are
amply sufficient for holding all the preparations a professional man
is likely to be able to collect in one year in the course of his ordi-
nary practice.

A glass vessel of the capacity of a gallon, with a large mouth
closed either with a cork enveloped in cerate-cloth, or a glass stop-
per, is very convenient for the object in question. The preparation
neatly dissected, may be advantageously attached by a thread, the
outer end of which should also be smeared with cerate ; or if the
preparation is lighter than spirit, as a portion of lung containing
air, the same object may be effected by attaching it to a piece of
glass.

In this way a great many preparations may be introduced into
the same vessel—indeed the vessel may be almost filled with them
without detriment, provided each is free, and not pressed against
by another, which is easily managed by using threads of different
lengths. This method, it may be added, is particularly well adapted
for sending preparations to England, on account of its economy,
the little space required, and its security. Using it, there is no
danger of the preparations being left dry and ruined by the capillary
action of threads sucking up the spirit, and draining the vessel, the
cerate preventing such action; nor is there any danger of atmo-
spheric air finding admission, provided the glass stopple or cork
with which the bottle is closed is covered with moist bladder firmly
tied down, and smeared with oil when dry; nor of any material

loss from evaporation. Here a caution may be given, that when bladder is thus used, the bottles should be placed out of the reach of rats, mice, and cockroaches, animals greedy of this membrane, and who attack it whenever it comes in their way.

Numbers written with a lead pencil on slips of strong paper, parchment, or wood, may be introduced with the preparations when they are numerous and there is any apprehension of mistake; which numbers will of course have reference to a descriptive list, that should accompany the preparations to England.

Relative to the making of dry preparations in hot climates, it will be sufficient to offer a very few observations. The unexperienced in these climates may fancy the task in question exceedingly easy, from a common and erroneous association of the ideas of proximity to the sun and parching heat. They will find it, however, a more difficult labour than can be imagined *à priori ;* and for this reason, that the connection of heat and dryness just now alluded to, is in most hot climates of rarer occurrence than the association of high temperature with a great degree of humidity. This latter happens when the wind sweeps on its way over a great extent of sea, and on its passage becomes loaded with moisture, as is the case with the S. W. monsoon along the coast of India, the S. E. or sirocco in the Mediterranean, and the sea breeze in the West India islands. During the prevalence of these winds, it is very difficult to dry any anatomical preparation, and impossible indeed, unless recourse is had to some helping circumstance, as exposure to the direct rays of the sun, or the dry heat of a charcoal fire.   On the contrary, when the atmospheric heat is accompanied with dryness, as it always is when the wind comes over an extensive tract of country, such as the land wind in India, the N. W. in the Ionian Islands, the S. E. on the Western shore of Southern Africa, then the making of dry preparations is very easy—exposure to the wind is by itself sufficient.   When dry, in every instance, the preparations should be varnished to defend them from the action of the atmosphere, and from the effect of vicissitudes in point of humidity, and then they should be carefully packed up in dried paper in a box of tight construction to be sent home by the first opportunity.   As dry preparations of morbid parts, with the exception of bones, are of comparatively little value, nothing that is particularly interesting, capable of being kept in spirits should be preserved in any other way.

It is not considered necessary to give any hints respecting the methods of making injected preparations.   Those who have sufficient zeal for anatomical pursuits to engage in this undertaking, will have no difficulty in carrying it into effect wherever they may be, with the knowledge which they must have acquired previously, and being possessed of the same apparatus that is requisite in temperate climates.

In concluding, it may be remarked, that, simple and easy as are the means described for making and preserving morbid anatomical

preparations in hot climates, they are quite adequate, and with ordinary care cannot fail to succeed. On this subject I can speak with some confidence from experience. During a period of eleven years, that I was stationed in the Mediterranean, in the Ionian Islands and Malta, they had a fair trial, and were found to answer perfectly ; of which, I may add, permanent proof is afforded by the specimens of diseased structure thus preserved, sent from thence, and which are now in the Museum of Pathological Anatomy at Fort Pitt.

XXXIV.

NOTICE OF A CASE IN WHICH THE ARTERIA INNOMINATA AND THE LEFT SUBCLAVIAN AND CAROTID ARTERIES WERE CLOSED WITHOUT LOSS OF LIFE.

Sir Astley Cooper, in the first volume of Guy's Hospital Reports, has given a valuable paper on the effects of tying the carotid and vertebral arteries in the dog and the rabbit, proving, that in the former animal both arteries may be suddenly and at the same time tied, without destruction of life, and that in the latter they may be successively tied with the same result—the circulation of the blood in the brain being continued by the admirable conservative process of anastomosis.

What Sir Astley Coopor has demonstrated as practicable in these animals, might be inferred analogically of man, taking into account the results of ample experience on the ligature of all the larger arteries singly in different instances for aneurism, successful beyond even sanguine expectation ; and a case which has come under my observation, confirms the justness of the conclusion, as clearly, I apprehend, as if instituted, were it possible, experimentally for the purpose. As the subject is very important in its physiological relations, as well as in its bearings on pathology, I shall describe this particular instance with some minuteness, although not so much in detail as may be desirable, owing to circumstances connected with the individual.

An officer of rank, aged about 55, distinguished for his services in the field during the Peninsular war, and who had been severely wounded in the chest at Waterloo, after good health for several years, had, when absent in England from his command in the Mediterranean, in the winter of 1831, an attack, which I was informed was considered rheumatic, attended with pain in the right shoulder, for which calomel and opium were largely prescribed. In September of the following year, he had acute catarrh, of considerable severity and duration, followed by chronic inflammation

with enlargement of the uvula and a granular state of the pharynx, of great obstinacy, attended with cough, and an impaired state of the general health. In the spring of 1834, when his health appeared to be improving, he noticed a peculiarity of alvine evacuation ; and scrutiny being made, it was found, that he voided daily a small quantity of purulent fluid, and which resisted treatment for rather more than six weeks. The summer of this year was spent in Switzerland, where his health improved. During the winter of 1834–5, he was in a valetudinary state ; subject to cough ; to frequent pain in the right shoulder; to occasional difficulty of breathing : the pulse was small and always quick ; rarely below 90°— sometimes exceeding 100° : the respiratory sound was natural ; the heart's action (judging from its sound and impulse) was rather tumultuous : there was emaciation, loss of strength, and depression of spirits—alarming his friends. For several months, his state fluctuated—on the whole apparently improving. He ceased to be immediately under my observation in March 1835—on my leaving the Mediterranean. In the June following, he returned to England by way of the Continent ; and in August, about three weeks after, he wrote to me, that he was then unusually well, but that on landing, he had been ailing a little, and had consulted Dr. Chambers ; and this gentleman, in a note with which he has favoured me, in reply to my inquiry relative to the then state of pulse, mentions that he "never could perceive any pulse from the time he first saw him." In September, he went to Brighton, and was taken suddenly and seriously ill with tendency to syncope and vertigo frequently recurring : then it is certain that no pulse could be perceived at either wrist. In October, when he had returned to town, I saw him twice, and the second time in consultation with Dr. Chambers and Dr. Hume. His general health was better than it had been at Brighton ; he experienced vertigo seldom, and syncope never ; he daily walked to his club-house in St. James' from an adjoining street, and played gently at billiards under caution. His disease now was clearly aneurism of the arch of the aorta, with an obstructed state (it might be inferred) of the great vessels arising from it ; for no pulse could be felt any where in the course of these vessels, neither in the neck, temples, axilla or wrists, and there was a throbbing pulse at the upper part of the sternum, and a slight prominence of the bone there to some little extent. Until the autumn following, I saw him generally once a month : he had fewer uneasy sensations—felt stronger and considered himself better ; the rising of sternum was rather extending, the abnormal pulsation not increasing. He then went into the country; he felt, I was informed, still better ; his general health was decidedly better. This was indicated by his wish to shoot—an amusement he was extremely fond of, but which, when applied to, of course I opposed ; strongly urging, in common with his ordinary medical attendant, his remaining very quiet, and in regard to diet living below par. On the 11th of January, 1837, that is, nearly a year and a half from the

cessation of the pulse at the wrist, he expired suddenly. That morning he left his friends to return to town, and felt and appeared unusually well : he traveled about 60 miles in a close carriage be- fore stopping to dine, and where he intended to pass the night. The last post or two he felt some pain in his shoulder, but, as I was informed, spoke lightly of it. According to his usage on quit- ting his carriage, he took in his hand a small writing case ; and shortly after arrival at the inn, sat down to dinner with an appe- tite, and the expectation of the removal of some uneasy feelings. He took some soup, and was in the act of taking some fish, when suddenly his jaw was observed to drop, and without uttering a word or a sound, he fell back in the chair dead.

A *post mortem* examination was made by a very competent sur- geon. The pericardium was found distended with coagulated blood, and a small fissure was detected in the aorta, near its base, through which the blood had penetrated. The heart was rather large, and its cavities large, but without distinct hypertrophy ; its valves natural. The arch of the aorta was, as had been inferred, the seat of a large aneurism, about six inches high, and about four inches wide, nearly filled with coagulum, dense, fibrous, and nearly white, occupying all but the inferior space, where there was a channel open by which the blood passed from the heart into the descending portion of the vessel. Where the vessel commenced its descent, there it recovered its natural dimensions ; but though of ordinary size, it was not perfectly sound ; there were some athero- matous deposits on it, and a few bony scales. All the great vessels rising from the arch were completely closed up at their origin. The upper portion of the innominata was open ; the right carotid and subclavian arteries also were open, but rather diminished in size. The left carotid, subclavian and vertebral arteries, as far as they were examined, namely to the extent of about two inches, were impervious, plugged up with lymph. The intercostal arte- ries at their origin (they were not examined farther) were large. The state of the cerebral vessels, in consequence of the rapid man- ner in which the inspection was made, was not ascertained : the aneurismal tumour was preserved, and through the kindness of the gentleman, who removed it, (Mr. Johnston,) I had an opportu- nity of examining it, and of satisfying myself respecting the most important points.

In this instance, probably, the aneurism was of slow formation, and was latent, as so often happens in this malady, not only many months but even several years : the interesting part of the case, how- ever, is the obstruction of the great arteries, arising from the diseas- ed portion, at their origin, and yet the supply of blood, not being cut off, clearly proving that new channels were established by anastomoses, by which a quantity of arterial blood was transmitted to the head and upper limbs, not only sufficient to maintain life, but enough to maintain it with the vigour of ordinary health. At no time, after the obstruction of the great arteries, was there any

paralytic weakness of the hands, no numbness even of the fingers, and no decided diminution of temperature. The intellectual organ performed its functions unimpaired, excepting there was occasionally some slight confusion of mind and a vertiginous feeling, especially in suddenly rising from the horizontal or reclining to the erect posture. All the senses were ordinarily unaffected ; the sight, the hearing, the smell, and taste, were commonly as acute as ever, and the general functions of the system were well performed ; so that, had not the aneurism burst into the cavity of the pericardium, life might have been continued with ease and enjoyment, and no doubt would have been so continued, had a channel for the passage of the blood been formed through the fibrinous mass in the aneurism instead of below it.

As the circumstances under which the post-mortem inspection was instituted did not admit of minute examination, much less of the injection of the vessels, the manner in which the circulation was carried on above the obstructed vessels can only be conjectured. The intercostal arteries, especially the superior, were probably the ones chiefly concerned in effecting a communication, either with the pervious principal arteries springing from the innominata, and their branches, or the probably pervious branches, given off by the left subclavian and carotid arteries.

As supplementary to this case, I shall briefly notice another, in which also there is reason to believe, that the same great vessels were permanently obstructed, as in the preceding, but with a train of symptoms and an event very different.

Hector M'Callum, of the 42d regiment, aged 36 ; twenty-two years a drummer in that corps ; notwithstanding intemperate habits, had good health, until the beginning of 1833 ; when he first experienced some difficulty of breathing; but he continued at his duty until the 18th of February, 1834. He was then admitted into hospital, at Malta, and I frequently saw him in company with the able surgeon of the regiment, Dr. Nicholson, (now Assistant-Inspector of Hospitals,) to whom I am indebted for most of the particulars of the case. The principal symptoms at that time were palpitation with tumultuous action of the heart, greatly aggravated by bodily exertion, with dyspnœa, aggravated by the same cause, occurring in paroxysms, during which he experienced temporary loss of vision, and occasionally syncope. No pulse could be felt at either wrist, nor in the brachial arteries, and indistinctly only in the carotid arteries, but strong in the femoral. The tongue was clean, the appetite good, the functions generally (independent of respiration) well performed ; yet he was feeble. He gradually became worse. In May, the dyspnœa was continually severe, often threatening suffocation, the face assuming a livid hue, with great agony of suffering. Now, no pulsation could be felt in the left carotid artery, and only very indistinctly in the right, without return of the pulse in either wrist. He died suddenly on the 2d of June.

Owing to aversion on the part of the widow to a post-mortem examination, the inspection was very partial and hurried.

Two pints of serum were found in the left pleura and three in the right. The lungs were not apparently diseased. The heart was somewhat larger than natural : its valves were pretty sound, even the semilunar of the aorta. The aorta throughout its arch was enlarged ; but only in a moderate degree, and its coats were more or less altered. Its inner coat was irregularly thickened and opaque, and the middle coat corresponding had become thinner, and of a heightened yellow tint. Thin plates of bone were formed in, or under the former, and atheromatous matter here and there in the latter, and in one spot matter of this kind, of about the bulk of a peppercorn, soft, brownish, poultaceous, had penetrated through all the coats to the external loose cellular enveloping tissue. The left carotid and subclavian arteries were completely obstructed, firmly plugged up with dense white matter, which it may be inferred was lymph. No trace even of the opening of the carotid from the aorta could be discovered, and a very slight trace only of the other, a little cavity a line or two deep, resembling the vestige of the aperture of the ductus arteriosus in the adult. It is to be regretted that in the hurried examination, and owing to interruption, the state of the arteria innominata, and of the right subclavian and carotid arteries, was not determined. The descending aorta was of its normal dimensions.

This second case in relation to symptoms is remarkably contrasted with the first :—in the one, a natural curative process appears to have been set up, to counteract the local evil ; in the other, the reverse of this process appears to have been in progress,—probably a closing of the larger arteries, without expansion and extension of their branches, or of the adjoining arteries, and consequently the production of a permanent impediment to the circulation in the brain and upper limbs, and especially in the former. If this were so, fortunately, it may be considered a very rare occurrence, and as it were the exception to the general rule. The common train of events, that which constitutes the rule, seems to be that which was witnessed in the first instance, and which has been so happily illustrated by the experiments of Sir Astley Cooper already referred to.

The subject of the occlusion of arteries is one of great interest and of more frequent occurrence, I apprehend, than is commonly supposed. I have met with many examples of it, after death, which during life were never suspected, and even in large vessels, and in soldiers of active habits, who died of acute diseases of short duration, totally unconnected with the local peculiarity. I am disposed to believe that whenever the middle coat of an artery becomes diseased, whether from atrophy, or change of structure, or deposition in it, of that matter which has been called atheromatous, or any other, there is invariably an effort, as it were, made to strengthen the part, by the deposition of lymph on both sides; and that accord-

ing to the rate of progress of the two processes, there is either a closure of the vessel, or an aneurism established : if the lymph is deposited rapidly, the former ; if the atheromatous matter, the latter : and also the latter, if the absorption or weakening of the middle coat from diseased alteration in its texture proceed with more speed than the deposition of lymph.   This I admit is hypothesis ; I wish it to be considered as such ; I venture to propose it, with the hope of calling attention to a subject. as I believe, of great importance, and with the hope of exciting discussion, which is always useful.

XXXV.

NOTICE OF A FATAL CASE OF RUPTURE OF THE HEART AND AORTA; WITH AN ACCOUNT OF SOME EXPERI-MENTS ON THE POWER OF RESISTANCE OF THE HEART AND GREAT VESSELS.

Thomas M'Garey, aged 42, about to be invalided on account of length of service and diminished activity ; according to a comrade, of " lonely habit," fond of walking by himself ; but well conducted and sober, and previously in good health,—was found dead early in the morning of April 14th, 1823, in a chalk-pit in the neighbour-hood of Chatham, at the foot of a precipice, between 50 and 60 feet deep,—the greater part of it perpendicular.   The body was ex-amined on the 16th, the weather was cool, and it had been kept in a cool place.   Externally there was no mark of violence or appear-ance of contusion.   The face and surface of the body generally were pale.   The pupils were contracted ; the limbs rigid.   The right inferior extremity was a little shorter than the left ; it was sup-posed that the thigh was fractured ; and on exposing the bone it proved to be so, at the neck, in an oblique direction just without the capsular ligament.   Nothing morbid was discovered in the brain. No ribs were fractured.   On opening the chest, a large quant ty of blood was found in the left pleura, about six pints, and a small quantity in the pericardium, about an ounce.   The blood was dark, of the colour of venous blood, and had separated into crassamentum and serum, and the serum was tinged red.   No lesion was detected either in the right auricle or ventricle, or in the vena cava ascendens, or descendens, or in the pulmonary artery.   The left ventricle also was uninjured ; but not so the left auricle and aorta, each was rup-tured.   The true auricle was ruptured in two places ; each opening was sufficiently large to allow the passing of the finger.   The aorta was ruptured just above the mouths of the coronary arteries ; and also at the upper part of its arch, a few lines inferior to the origin of the left carotid and subclavian arteries.   The laceration at its base

was nearly circular, and through all the coats, and were it not for a narrow strip, of about two or three lines which remained entire, the vessel had been completely torn in two; even the external loose cellular membrane was divided to the extent of about half its circumference.   The rupture at the arch was oblique, also through all the coats, and rather more than one half the circumference of the vessel.   There was no blood in either of the cavities of the heart. Both ventricles were firmly contracted; and the valves of the aorta and pulmonary artery were unusually large and perfect.   The aorta was carefully examined throughout its course; excepting two or three opaque spots on its inner membrane, without sensible thickening, it exhibited no signs of disease, and appeared to be of usual firmness.   Where the vessel was ruptured, in each place the coats seemed sound; nor did the ruptured auricle exhibit any indications of organic disease.   The lungs were free, and crepitous; their substance was redder than usual, and contained more blood than usual. The trachea and bronchia were bright red internally, and coated with blood, probably owing to hemorrhage from the lungs. Nothing abnormal was found in the cavity of the abdomen; neither the spleen nor liver, (of all the viscera most liable to laceration from falls) was appreciably injured.   Both the stomach and bladder were found moderately distended, one with yellowish chyme, which had no smell of spirits,—the other with pale urine.

According to the report of those who discovered the body, and of those who saw it before it was moved, it lay on its back,—the feet nearest the precipice, in a diagonal position.   The limbs, it is said, were rigid, and every part of the body cold, excepting the left side of the chest which was pretty warm.   No part of the dress was torn. The ground was firm, bare chalk.

These particulars of this very uncommon case were noted down at the time.   Taking them all into consideration, it seems most probable that the soldier lost his way in a lonely night-walk, and coming to the brink of the precipice stepped over and fell on his feet, or rather perhaps on the one (the left), the neck of the thigh-bone of which was fractured.   On this supposition the unbruised state of the integuments, the uninjured state of the clothes, and the oblique or diagonal position of the body in relation to the foot of the precipice, as well as the kind of fracture, seem to be tolerably accounted for.

Relative to the fatal injuries, the rupture of the left auricle, and the double rupture of the aorta,—I apprehend, that, reasoning *à priori*, conjectures only can be offered as to the manner in which they were produced; and especially taking into account a fact, which occurred about the same time, and of the accuracy of which I am well assured, viz. that a lad fell down the same precipice and alighted on his feet without sustaining the slightest injury.   But, however explained, whether on the supposition that the lacerations were owing to muscular action,—to the violent contraction of the left auricle and ventricle; or, to other causes in conjunction with such

action,—the effects themselves seemed to demonstrate, that where they took place, there was a greater tendency to yield and to rupture than elsewhere, at least in this particular case.

Thinking it probable that elucidation might be obtained by experiment,—I instituted at the time some trials directed to test the strength of the parts concerned, which I shall now detail, with a few additional ones since made.

Experiment 1.

Tied the descending vena cava of a sheep recently killed, and through an opening in the ascending vena cava, introduced the tube of a powerful injecting syringe, and secured it firmly by ligature. Forced water into the right side of the heart ; a large quantity was introduced ; the vein, auricle, ventricle and pulmonary artery became very much distended, as did also the lungs, and a watery exudation took place from their surface, without any sensible breach of continuity of surface. Continuing the forcible injection of water, the right side was at length ruptured ; the rupture took place in the sinus venosus close to the auricle, and was large enough to admit the finger.

Experiment 2.

Made a similar trial on the left side of the heart of the same animal, by injecting water through one of the pulmonary veins, close to its termination, having previously secured by ligature the aorta, below the arteria innominata. The auricle became greatly distended, the ventricle in a less degree, and the aorta still less. Rupture took place in this instance much more readily than in the former ; the auricle was the seat of it ; it was sufficiently large to admit the finger.

Experiment 3.

Made a similar experiment on the aorta. It expanded but little ; it was ruptured about one inch from its origin, and to the extent nearly of one half of its circumference. The pipe of the syringe was introduced just below the arteria innominata. The semilunar valves performed their function perfectly, and were found uninjured.

Experiment 4.

Made a similar experiment on the pulmonary artery. Introduced the pipe of the syringe into one of its main branches, and secured the other by ligature. It dilated more than the aorta, and considerably ; its rupture took place before much force had been employed ; it was transverse, about one half the circumference of the vessel, and through all its coats, and about half an inch above the valves, which acted well and were uninjured.

Experiment 5.

Tied the pulmonary artery, and descending vena cava, and forced

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in water through the ascending vena cava; when but little force had been used, and when the auricle and ventricle were not very much distended, a rupture took place, which on examination was found to be of the fossa ovalis; the rent was oblique; the membrane was torn at its margin, with some muscular fibres with which it was there united.

Experiment 6.

Tied the ascending vena cava, just before its termination in the heart; and, having introduced the pipe of the syringe, through a small opening made in the vessel just above the liver, and secured it well by ligature, an attempt was made to burst it, but in vain, although all the force of a strong man was applied to the piston, aided by all the pressure I could make with the hand on the dilated vein: its dilatation was considerable, but not to the extent that might have been expected.

Experiment 7.

This and the following experiments were made on the human subject. About four hours after death, from tubercular disease of cerebrum, with softening of its substance, complicated with tubercles and a few small cavities in the lung, in the instance of a young soldier, aged 26, water was injected through one pulmonary vein, close to its termination;—the other pulmonary veins, and the arteria innominata, and the left carotid and subclavian arteries, as well as the aorta just below, having been secured by ligature. The left side of the heart became greatly distended: after much force had been applied a rupture took place—the auricle, its sinus, burst close to the entrance of one of the pulmonary veins.[1]

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[1] In the above, and in many other instances noticed in this work, the post-mortem examination was made sooner than is usual. In all such cases means were taken to determine previously the reality of the fatal event—as by making a small incision through the cutis, and puncturing the fibres of the platisma myoides (a muscle, which is probably very retentive of vital contractile power, judging from the analogy of the panniculus carnosus) and by exposing the eye to light. To the pathologist, conversant with necroscopical research, such precautions may appear superfluous; he may say, Who ever knew an example in hospital experience, of a body supposed to be dead, and removed as such from the ward to the dead-house, either reviving, or affording any indications of remaining vitality? I never witnessed an example of the kind, nor ever heard of one well authenticated, excepting in bodies which had died of that mysterious disease, cholera, in which in a few rare instances, it is well established, that particular muscles have acted after the extinction of life. Notwithstanding, although all experience supports the foregoing conclusion, yet as apparent death—suspended animation—is within the limits of possibility, and keeping in recollection what happened to the illustrious Vesalius, it is at least in accordance with proper feeling, and a satisfaction, to employ the tests alluded to; and the popular advocates of measures to prevent what is held up to horror—too early burying, on the supposition combated, that suspended animation and apparent death is not uncommon, would act more consistently with their views, if, instead of pro-

Experiment 8.

About four hours after death from chronic dysentery, complicated with a few tubercles in the lungs, and two small cavities, in the instance of a soldier aged 28 years, after tying the aorta below its arch and its main ascending branches, injected water by one of the pulmonary veins, the others being left untied. The left auricle became exceedingly distended, and the coronary arteries. The ventricle and aorta were less distended. The lungs were very much distended, and there was an oozing of water from their surface; when a portion was pressed gently between the hands, the water actually dropped from it, and yet the investing pleura appeared to retain its integrity. After employing much force, a rupture was with some difficulty effected. The water rushed out through the trachea, indicating the bursting of a pulmonary vein in the substance of the lung.[1]

Experiment 9.[2]

Next secured by ligature the pulmonary veins, just before their termination, and the syringe being applied to the same vein as in the former instance, the subject being the same, and the other circumstances the same, water was injected, until a rupture occurred. It took place opposite the fossa ovalis, in the thickest part of the sinus, close to the auricular-ventricular passage; the rent was sufficiently large to admit two fingers. Much force was required to effect it, and it was preceded by much dilatation.

Experiment 10.

From the pulmonary vein, the pipe of the syringe was removed to the descending aorta, just where it begins to descend, and was secured by ligature. On injecting water, the vessel gradually became much dilated; at first the semilunar valves appeared to perform their functions well; but as the dilatation increased, water began to

posing that bells should be hung up within reach of the interred, or attached to their persons, they recommend the puncturing of some irritable part, before the body is committed to the coffin.

[1] It was curious to see in this instance, how the lining membrane of the left side of the heart, and of the aorta, above the ligature, were intensely stained red; whilst the portion of the latter below it, was colourless; affording strong proof of the dyeing power of the colouring matter of the blood (the left side of the heart contained some blood) when in solution.

[2] Previous to commencing this experiment, a small opening was made in the pericardium, and the contained fluid was carefully removed by means of a sponge, and then the opening was closed by a ligature. At the end of the experiment, it was re-opened and examined; notwithstanding the distended state of the auricle and ventricle, and of the coronary arteries, and that nearly half an hour was passed in making the trial, owing to delays, not more than three or four drops of water were found collected, the result of exudation. This result may be adduced as a confirmation of the conclusion arrived at, heretofore, relative to the source of the fluid so frequently found in the pericardium after death.

flow out through the ruptured auricle, augmenting with the disten-
sion, till it flowed freely.

Experiment 11.

Twenty-nine hours after death, from pulmonary consumption,
with numerous cavities in the lungs, and one of vast size, in the
instance of a soldier aged 29, introduced the pipe of the syringe into
the right internal jugular vein, not having tied any vessel, except-
ing the opposite veins in the neck,—the cavity of the abdomen
having been carefully laid open, and also of the chest partially by
the removal of the sternum and of the costal cartilages. After a
certain quantity of water was injected, it flowed out through the
trachea, probably through ruptured vessels in the tubercular exca-
vations. The lungs, the liver and spleen, were rendered tense, and
there was an exudation of fluid during the continuance of the pres-
sure, not only from the surface of the lungs, but apparently, gene-
rally, in the cavity of the abdomen from all its contained viscera.

Experiment 12.

The result was similar, when water was injected into the aorta,
just above its division into the common iliac arteries. As the artery
became dilated, the semilunar valves performed their function less
perfectly, and the flow of water through the trachea increased.

Experiment 13.

Twenty-five hours after death from peritoneal inflammation, in
the instance of an insane soldier, aged 44 ; after examining the brain,
and securing the vertebral arteries within the cranium, and the
carotid arteries in the neck, previous to opening the chest, introduced
the pipe of the syringe into the aorta just before its division into the
common iliac arteries, and having secured it by ligature, (all the
abdominal viscera remaining undisturbed,) injected water as before.
It was necessary to inject a very large quantity, and to employ great
force, before a rupture was effected. The ruptured part was detect-
ed with some difficulty ; it proved to be the left emulgent artery,
about a quarter of an inch from its origin. Its middle coat was
lacerated in two places, in the direction of the fibres composing it,
and also the inner coat. There was no perceptible opening in the
outer cellular. The course of the aorta, even through the chest,
was distended with water, and indeed the whole of the posterior
mediastinum was so distended, and pretty much had found its way
into the pleuræ. The left ventricle and auricle were empty, ex-
cepting that the latter contained a little blood, showing that the
semilunar valves had performed their function to perfection.

Experiment 14.

Thirteen hours after death from tubercles and cavities in the
lungs, complicated with diseased omentum, enlargement of the liver
and severe ulceration of the large intestines, in the instance of a sol-

dier aged 33 ; after examining the brain and tying the jugular veins, the pipe of the syringe was introduced into the right internal jugular vein, below its ligature, and water was injected; considerable resistance was almost immediately experienced, giving the idea, that the right cavities of the heart were obstructed with coagulated blood. After the syringe had been emptied two or three times, great force was required to introduce more water ; the vein towards the heart was amazingly distended, and shortly, the pressure being continued, it became ruptured ; the effect took place just before the entering of the vessel into the anterior mediastinum. It seemed as it were rather the separation of its fibres with distension, than the breaking of them from distension.

Experiment 15.

The vertebral arteries, where they enter the cranium, having been secured by ligature, and the left carotid artery in the neck having also been tied, the pipe of the syringe was placed in the right carotid artery, and water was injected,—the cavity of the chest and abdomen remaining unopened. Very little resistance was experienced till a large quantity of water had been injected, and even when an enormous quantity had been introduced, the resistance was not considerable. The experiment was discontinued when the flow of water from the neck, the nostrils and the inside of the cranium, was almost equal in quantity to that which could be thrown in. This flow of water seemed to be from exudation. The body was amazingly distended, and exhibited a very singular appearance,—the penis turgid, the mamillæ dilated, the papillæ erect, the trunk and limbs tense, rounded and filled out, which (with the exception of the abdomen in which some fluid was effused in connection with the diseased omentum and enlarged liver,) were previously shrunk and emaciated.[1]

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[1] Hales, in his Hæmastatics, details many curious and instructive results of experiments on the injection of water into the vessels. In an experiment on a dog, he states : " If the warm water was continued thus" (in a column 9½ feet high) "flowing into the artery for half an hour, or two hours, all the parts of the body would, during that time, be continually swelling bigger and bigger, so that there would be a universal dropsy over the whole body ; both the *ascites* and the *anasarca :* the salival and other glands were greatly swelled, and the mouth and nose filled with mucose slimy matter, which flowed from those glands; the ubera were much distended by the filling of their fat vesicles, as were also all the fatty vesicles of the body. All the muscles were swelled, and the interstices of their fibres filled with water; and some of them were by this means washed white. All this was effected with a force of water no greater than that of arterial blood in its natural state."—Stat. Essay, 3d ed. vol. ii. p. 113. With these results, the above, and what else was observed at the same time, so accorded, that in reading the one, I found described the other, substituting perhaps for fatty vesicles when speaking of the ubera, their erectile vascular tissue, and for the same term generally, cellular tissue, or the subcutaneous vascular.

Experiment 16.

The sternum having been removed and the pericardium laid open, water was again injected into the right carotid artery, other circumstances remaining the same. The arch of the aorta, as much as could be seen of it, was greatly distended: but the left ventricle was not at all distended. No force that could be used could rupture the aorta,—the exudation continuing. Laid open carefully the left ventricle so as to be able to apply the end of the finger to the semi-lunar valves. Water was again injected forcibly, the aorta was again distended, and there was only an oozing of water from the ventricle: the valves were felt very tense, and, it may be inferred, completely closed the mouth of the vessel.

Experiment 17.

I shall give one trial more, in which—twenty-four hours after death from tubercles in the lungs, with very large cavities in the left lung and extensive ulceration of the large intestines—the pipe of the syringe was introduced into the right carotid artery, before the calvaria was removed, and after tying both jugular veins, and both vertebral arteries, close to their origin, and the left carotid in the neck. As water was injected, all the integuments of the neck, face, and head became distended, especially of the right side, and at length, in a very remarkable manner, much surpassing the greatest swelling I ever witnessed in the most severe cases of erysipelas, or of confluent small pox. The eye-lids, nose, lips, were protuberant and tense; the eyes closed; the mouth opened, from expansion of the lips; the tongue enlarged. Water exuded from the eyes, the nostrils and mouth; and, it would appear, from their surface generally. The experiment was discontinued, when the water which flowed out from various parts seemed to be nearly equal in quantity to that which was injected. On examination no large vessel could be found ruptured. On removing the calvaria, a strongly marked contrast was observable between the state of the exterior parts and of the interior, as, perhaps, might, *à priori*, have been expected, considering how the latter were encased in unyielding bony parietes. There was only a very little fluid effused in the cellular tissue of the pia mater; merely a moisture between the arachnoid membrane and the dura mater, and only a moderate quantity in the lateral ventricles and at the base of the brain: and the fluid so effused was probably not water which had been injected, but which had previously existed there; this is inferred from the circumstance that there was a larger proportion in the left lateral ventricle than in the right, notwithstanding that the pia mater of the former side throughout was red,—its vessels containing blood as usual, whilst that of the right was perfectly colourless,—all the blood having been washed out of its vessels. It is hardly necessary to observe that no vessel was found ruptured within the cranium.

The results of these experiments, in the way of illustration, as applied to the case with a view to which they were instituted, are in no wise clear and satisfactory; but, on that account I have not thought it right to keep them back, believing that as they have been faithfully detailed, they may not be altogether unacceptable or without use, especially in connection with the important and obscure subject of aneurism.

Considering them generally, they are, perhaps, less uniform than might have been expected; and hardly favourable to any general conclusions being deduced from them, excepting, indeed, the following:—1st, That the power of resistance possessed by the heart and large vessels, independent of any vital properties of endurance peculiar to them, is enormous;[1] and, 2dly, that there is much variation in point of strength in the same parts in different instances.

I have alluded to the rareness of the accident, which induced me to institute these trials. Referring to authors, I can find only one clear instance on record of a similar kind, that is, of rupture of any part of the heart, from a force acting through the medium of other parts, on this organ, apparently free from disease and in its perfectly normal healthy state. The case is noticed by Portal, in his Anatomie Medicale;[2] it was of a young man, over part of whose chest, the wheel of a cart loaded with stones passed : death was almost immediate, and the left auricle was found open. Chaussier, who examined the body, attributed the effect of the rupture to compression of the aorta by the wheel, and to over-distension of the auricle with blood, the consequence of that obstruction. It is true, that very many other cases of rupture of the heart are recorded,—as accidents, but they are either not sufficiently clearly described, to be depended upon, or there is reason to suppose that the heart was either crushed or else wounded, or that the part ruptured was previously diseased. The authors I have chiefly consulted have been, Senac, Morgagni, Haller, Portal, and Laennec. On the comparative strength of different vessels, besides Hale's very ingenious work already referred to, I have consulted Clifton Wintringham's " Experimental Inquiry on some parts of the Animal Structure," published in 1740, a few years after the Statical Essays of the former ; but although having great pretensions to accuracy in his manner of giving the details of his experiments, I must confess I have not been able to place sufficient confidence in his results to use them.

[1] Vide Hale's Hæmastatics.—The results of his experiments on the above point are very striking. Referring to them, he says,—" We see in these instances, the great strength of the coats of these vessels ; what great reason have we, therefore, with thankful hearts to say to our Creator, as holy Job did, when he contemplated on the wonderful frame and strength of his body, Job x. 11, *Thou hast* not only *fenced me with bones and sinews,* but hast also effectually secured the vital fluid in such strongly wrought channels as are proof against its most lively and vigorous sallies, when either agitated by the different passions, or by strong and brisk actions of the body."—Op. Cit. vol. ii. p. 156.
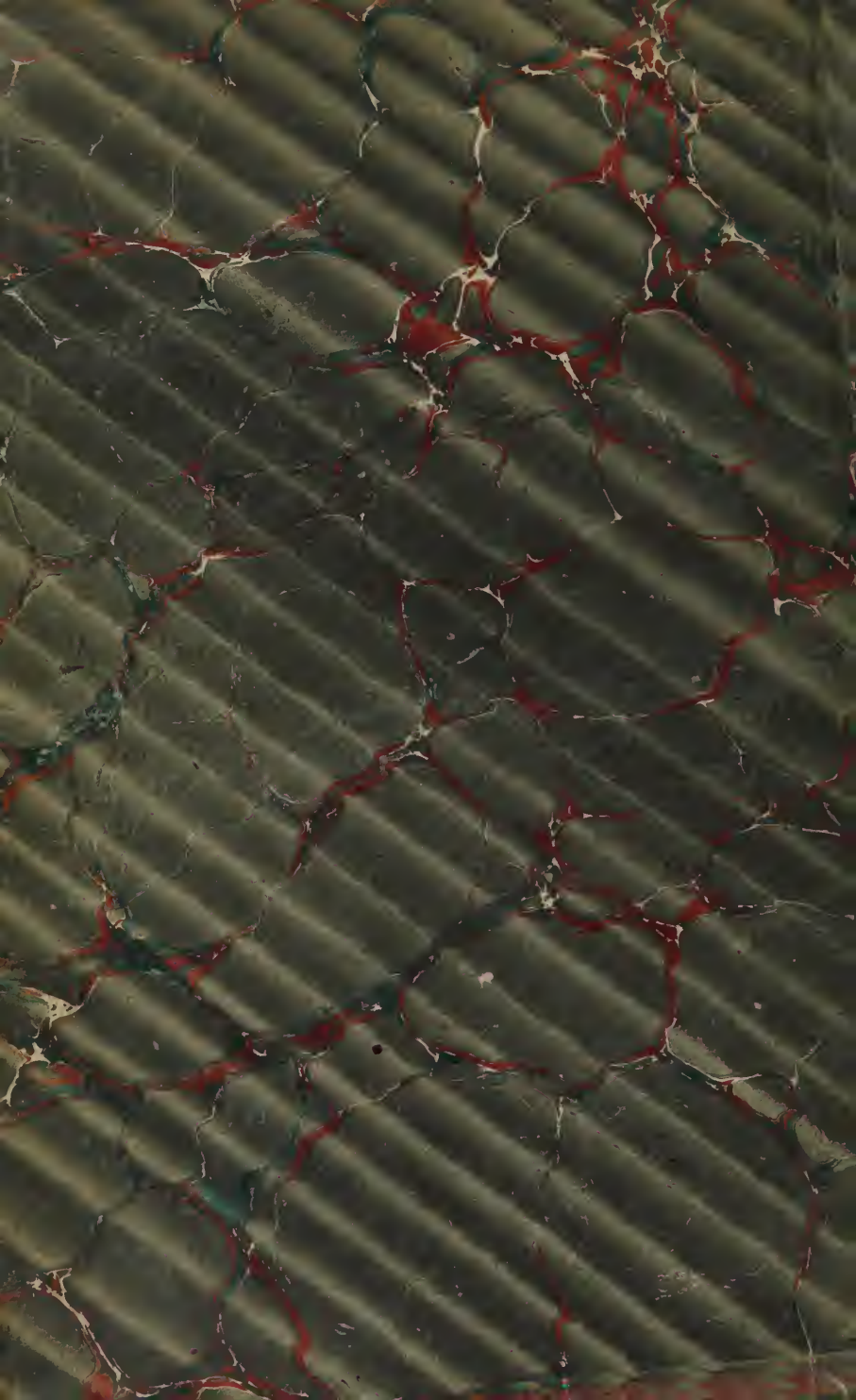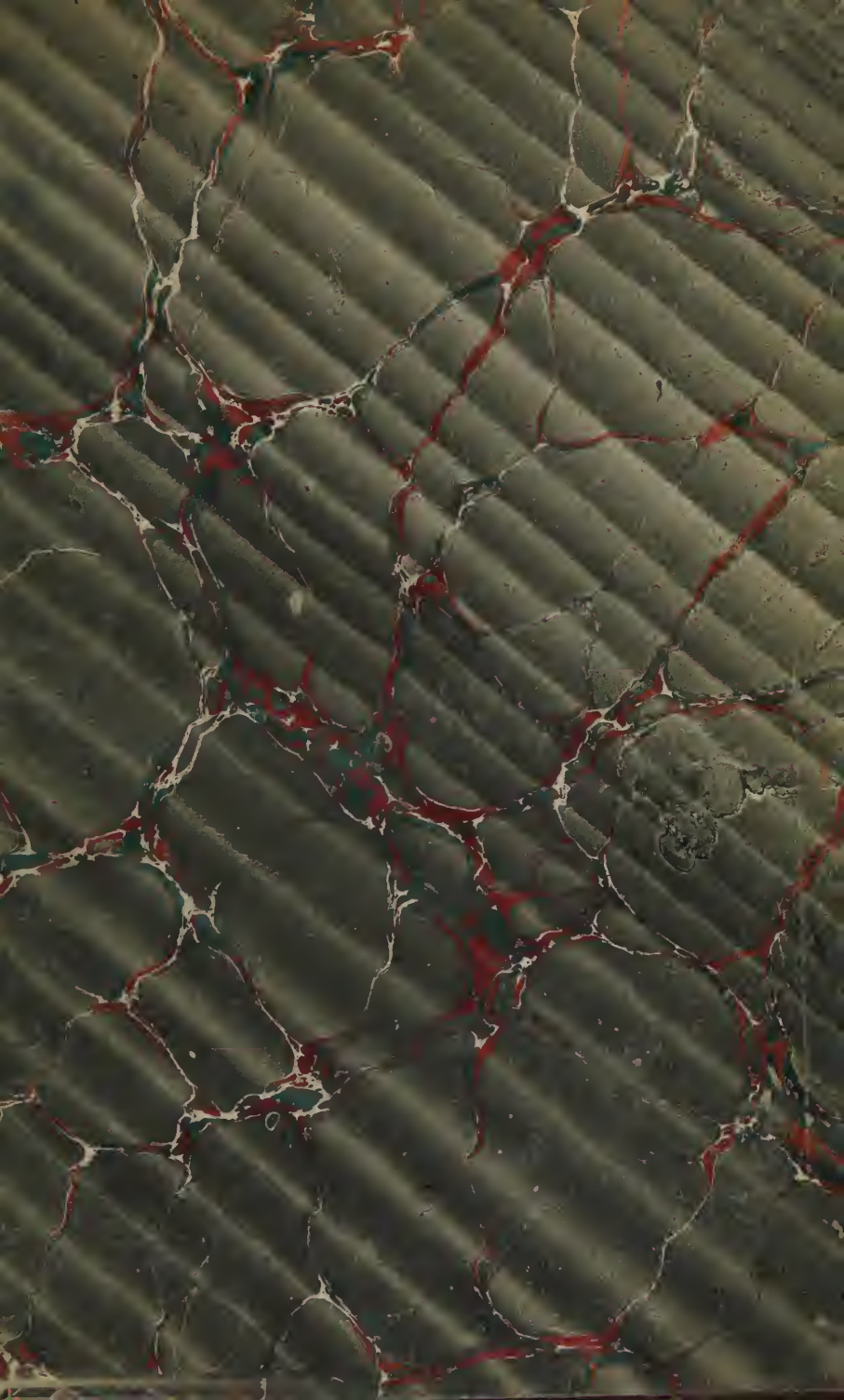[2] Tom. iii. p. 97.

In a previous page, a table will be found of the specific gra-
vities of different textures, formed from experiments conducted with
some care, and which I believe to be tolerably accurate.   It was in
comparing some of my results with his, that my faith in his correct-
ness was shaken.   According to him (to mention only one point),
there is a marked difference in the specific gravity of the descend-
ing aorta and of the vena cava corresponding, gradually diminish-
ing with age: thus, the specific gravity of these vessels in a young
man appears from his details to be, as 1060, and 1101,—the former
the artery, the latter the vein; and in an old man as 1098 and 1105.

This difference I have not been able to discover.   Very recently
I have carefully tried the specific gravity of each vessel, belonging
to a man aged 30;—the artery was 1060, the vein 1061.   They
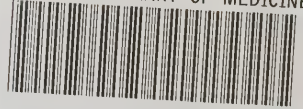were similarly treated in every respect preparatory to weighing.

Clifton Wintringham's results are so uniformly consistent,—they
all support each other so well, and are in such perfect conformity
with his conclusions, that it is rather difficult to avoid the inference
that he formed an hypothesis in the first instance and then made
experiments to support it,—notwithstanding the passage from
Leibnitz inserted in the title page of his book—*Ex iis, quæ experi-
mentis constant, ducere tentimus quicquid potest, antequam in
hypotheses liberiores expatiemur.*

THE END.